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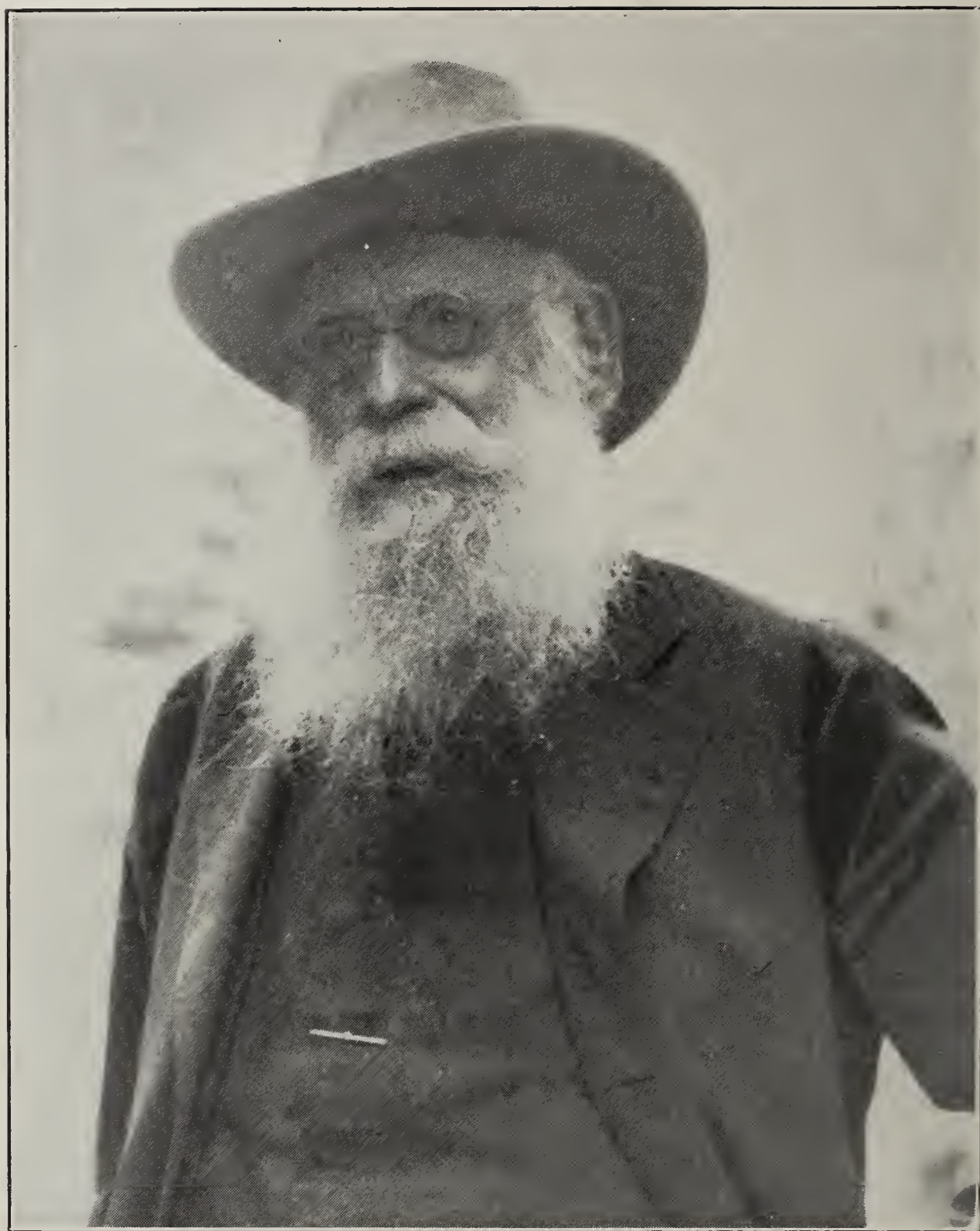
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No. 1

THREE GRAND DISCOVERIES OF LIFE¹

By CHARLES KEYES

With that recent astonishingly deep lowering of life's record in the geological column biotic evolution takes on new aspect. Sudden bursting forth, at the beginning of Cambrian time, of all of the main organic types now living finds ample explanation in potential rather than real appearances. Beginnings of organic being are pushed immeasurably back into the abyss of time.

In the consideration of evolutionary problems popular fancy dwells most appealingly upon the unearthing of missing links. Each personal interest oftenest focuses fondly upon the possible bridging of the chasm between man and monkey. Spectacular indeed this is; but it has little attraction for those best equipped to accomplish it. Biologically relative small importance attaches to this hypothetical man-ape. His one-time existence is now taken for granted, with faint curiosity aroused actually to dislodge him from under the dusts of the milleniums.

It is the fundamental changes which life undergoes in the course of time that are most illuminating and most sought. In viewing life's trek through the ages many are the missing links to be brought to sky. There are many gaps far more difficult to span than any of those near man. There are grave crises in organic expansion wherein growth and change are greater in short

¹ Paper read before the Geological Society of America, Amherst Meeting, December 28, 1921, under a somewhat different title.

episodes than those which take place in the millions of years that lie between.

Earth students sometimes marvel that when life on our globe is commonly conceived to start, at the beginning of Cambrian time, it appears to spring forth in great profusion, already nine-tenths differentiated, that in Devonian times vertebrate animals so suddenly rush upon the stage as to seem to be special creations, and that, in our own day, actual control of individual life span is in sight.

It is not by paleontological contemplation that these mysteries are unravelled. From the strictly geological angle little hope is held out for a solution of these novel problems. To the zoologist alone the phenomena do not give up the secret of their being. Only concerted action on the part of all three offers clue. At first purely speculative working hypothesis soon assumes plausibility, and then through accumulation of supporting facts the plan takes on semblance of established theory. Finally realism supplants the post of fiction.

Most momentous events connected with life's career are those three great crises in biotic development, the discovery of the bottom of the sea, the introduction of the back-bone, and the domestic use of fire and all that it means in turning awry the set course of nature.

Although little regarded among men amongst whom it was written the exposition of life's discovery of the bottom of the sea seems one of the master thoughts in American philosophy. Penned by modest zoologist upon apparently a strictly biological theme, but unfortunately published in a geological journal for the special benefit of paleontologists, it entirely missed its high mission. Not only was it utterly lost on the fossil brethren, but being published in an out of the way channel it completely failed to gain the attention of biologists among whom it should have found lodgement.

This remarkable essay had birth under very unusual circumstances. Possibility of the existence of a thick pre-Cambrian succession of sediments was occupying the center of the geological stage both in this country and the world. Under the title of Algonkian the Federal Geological Survey had proclaimed with great *éclat* the recognition, beneath the known Cambrian section, of an entirely new system of sedimentary rocks which, it was hoped, would prove to be comparable to Murchison's and Sedgwick's

notable discoveries, half a century before, of the Siluria and Cambria in England.

In this connection the crystalline complex of the Piedmont Plateau, in Maryland, where our zoologist was homed, was under especial surveillance. It was fondly expected that some of these rocks which had always been considered as among the very oldest parts of the Archean *massif*, if not actually a section of the primeval crust, might prove to belong to the so-called Algonkian sedimental column. At this very time under the ægis of the Johns Hopkins University, at Baltimore, these crystalline schists were subject of intensive study. Some of the results published started wide and warm discussion through the world.

With this astounding setting before him, that at the beginning of Cambric time biotic types were already so widely and fundamentally differentiated, and the further anticipation of great extension of the life record into time before Cambric, the nature of the pre-Cambrian life at once attracted the attention of Prof. W. K. Brooks, foremost zoologist and evolutionist of his day. Gathering from the geologists on the Piedmont Plateau, what he could concerning the physical setting of pre-Cambrian sedimentation, and the character of the earliest Cambric faunas, he began to speculate from the zoologists' viewpoint upon the aspects of life in these early times. His meditations crystallized into that masterly essay on the origin of the oldest fossils and the evolutionary significance of life's discovery of the bottom of the sea.

A supporting companion memoir by one of the Hopkins geologists, on the pre-Cambrian rock-section, was originally intended to go with the Brooks' essay. This, however, by curious decree of the Fates never materialized. The Piedmont Plateau schists turned out not to be Algonkian at all, nor even pre-Cambrian in age. They were found to be only highly metamorphosed representatives of the same Paleozoic strata, of Cambric, Ordovician, and Silurian ages, that were exposed unaltered farther west in the Appalachian Mountains.

The lowering of the life record into the abyss of time was not yet a reality, but, as it eventually proved, was a distant accomplishment a quarter of a century later. A vast pile of pre-Cambrian sedimentaries resolved itself in the Lake Superior region and in the Canadian Rockies. Instead of a single such system, as had been fancied, two great eral rock successions developed, each com-

parable not to the Cambrian, or Silurian, section alone but to an equivalent, perhaps, of the entire Paleozoic column. Moreover, a third such sequence below all the others was foreshadowed before the truly Azoic was to be regarded as reached. On the north shore of Lake Superior life remains were disclosed low down in the older of the new systems, at a time-level twice as remote as that which elapsed between the deposition of the basal Cambrian beds and the sedimentaries of our day. So the biotic effects of the discovery of the bottom of the sea still held good despite the fact of the removal of the immediate field of its testing.

When Darwin composed his *Origin of Species* no sedimentary section older than that of Cambrian age was known. He astutely observed that if his theory be true "It is indisputable that before the earliest Cambrian stratum was deposited long periods elapsed, as long as, or probably far longer than the whole interval from the Cambrian age to the present day; and that during these vast periods the world swarmed with living creatures." Although he could give no satisfactory reason at the time to the question why no fossiliferous deposits of this early period had been found, and notwithstanding the fact that he realized in this circumstance the gravest objection that could be urged against his hypothesis, recent discoveries in America carried the life record backward far beyond his most sanguine expectations. It was the bridging of this early gap that made the dissertation on life's discovery of the bottom of the sea so novel, so pertinent and so fascinating.

Brooks once emphasized the factors of the rapid intellectual development which has taken place among the mammals since Mid Tertiary times, and the abrupt changes which have transpired in both animals and plants when the land fauna and the flora were established, which were well known, but the circumstance that the discovery of the bottom of the sea by life initiated a much earlier and probably more important era of rapid development in the forms of animal life was never before pointed out.

Next to original creation of organic matter settlement of organisms upon the floor of the ocean was probably the most momentous single event in the entire history of life on our planet. The simplicity, abundance, freedom from danger, and the lazy conditions which primitive pelagic life enjoyed quickly changed in those regions where crustal upheaval occurred and where the waters became relatively shallow, to complexity, scarc-

ity, hardship, and fierce struggle for existence. Forms grew larger. Certain ones sustained themselves to the disadvantage of others. These were then able to turn their energies to growth and reproduction. Strong competition on the sea bottom or towards the shore soon gave rise to variant types, many of which persist to the present day.

With the further occupation of the clear, shallow, epicontinental seas hard parts developed and gave strong impetus to greater diversity. It is, then, not now so surprising as it once was that Cambrian life should burst forth in such boundless profusion. At that time the span of evolution was already twice or thrice as long as from the beginning of the Cambrian age to the present time; perhaps somewhat longer but certainly not eight to ten times longer as often surmised. Already hard parts of organisms, developed far down in the pre-Cambrian column were unearthed lately beneath even the horizon of the once famed but mystical Eozoan.

As Brooks astutely remarked, we are not to think of the populating of the bottom of the sea as a physical problem, but as a discovery and colonization, very much like the colonization of islands. This first bottom life was not pelagic life approaching the shore and occupying the littoral, because there the sediments were highly detrimental to such life. It was far enough from the coast for the waters to be free from sediments and deep enough for the pelagic food supply above it to develop without restriction. It was the deep zone around the islands and continents.

The first faunas which became established in this deep zone may have been the ones which persisted through the ages and the ones which may have given rise to modern animals. At all events, this deep zone was the birth place of the fauna which has survived, and it was the ancestral home of all the higher metazoa. After the foundations of the bottom fauna were laid it must have become very difficult for new forms to establish themselves. Thus evolution proceeded along lines which resulted in elaboration and specialization of the types already early established rather than through the introduction of new branches. The great groups of animals were rapidly developed from pelagic forms. They increased in size and developed hard parts. This was, indeed, perhaps the most important step in all organic evolution.

The sudden bursting forth at the beginning of Cambrian time of

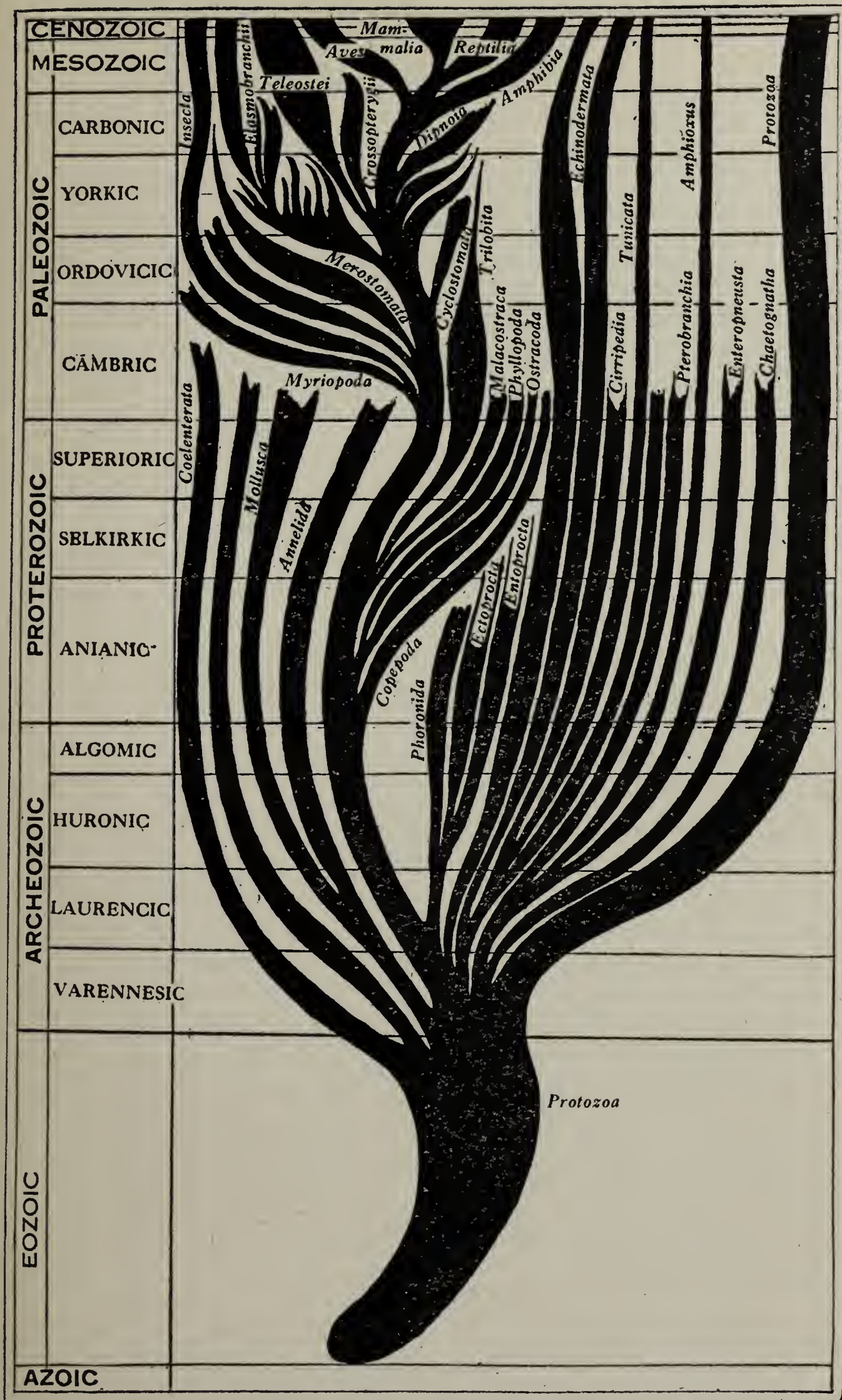
all of the principal types of life is, doubtless, for other reasons, more apparent than real. Our taxonomic notions of class distinctions are based mainly upon our familiarity with the land faunas about us, in a single branch which has come up to us from Cambric times. When the inferior types shall have been more equably considered it may be found that many taxonomic ranks will have to be raised. When this shall have been accomplished the singularly isolated branches which have traversed the ages will be doubtless discovered to be as complex and as ramifying as that branch of which man is the crowning glory.

Life's discovery of the bottom of sea narrows vastly the inferential span of its existence that is a necessary consequence of the conception that life was already nine-tenths differentiated at the beginning of Cambric time.

Of the many missing links in organic evolution, as disclosed by carrying the Darwinian theory to its logical conclusion, that of bridging the seeming bottomless chasm between the vertebrates and the invertebrates was always least responsive to revelation. This break long stood easily *facile princeps* among all difficulties. For half a century after the appearance of Darwin's epoch-making work the derivation of the back-boned animals resolutely withstood all attempts at satisfactory solution. The primitive Amphioxys, a simple fish-like organism having a well defined notochord but no bones, was long believed to fill the gap, but it really served, as we now know, to obscure the facts rather than to elucidate them. Then, too, among zoologists generally the annelid theory was so long and so strong in the forefront as to crowd out all else.

It is a singular fact that the paleontologists did not rise to the occasion and flash a ray of light into that dense darkness which the zoologists could not penetrate. The vast expansion of the trilobites in Cambric times should have at least aroused suspicion that the clue might be found in or near such a group. But the trilobites belonged to a class long since extinct, their internal structures were unknown, and their closest living kin was supposed to be a horse-shoe crab.

It remained for a demure professor of one of our lesser American colleges to find the key. William Patten, of Dartmouth College, was especially interested in the embryology of the Arthropods, that great class comprising the insects, crustaceans, and



EVOLUTION OF LIFE

spiders. Because of the fact that this was the largest and most highly specialized group of invertebrates zoologists were singularly unanimous in opinion that it was too specialized possibly to give rise to any new types.

When working upon the development of the eyes of arthropods Patten found that the fore-brain of an embryo scorpion was gradually covered by an overgrowing fold of skin that converted the brain into a hollow vesicle. During the process one or two pairs of eyes were transferred from the outer surface of the head to the blind end of a median tube that projected from the membranous roof of the brain. This procedure was unique among invertebrates; but it was so exact a counterpart of what took place in the formation of a rudimentary pineal eye of vertebrates as to indicate some intimate genetic relationship between the two groups.

"To test what at first sight appeared to be so improbable, a careful study of the anatomy and development of several types of arachnids was made, and, much to our astonishment it was found that the brain of the arachnids resembled that of the vertebrates in its general shape, in its subdivision into several regions, in the general nature of the functions performed by these regions, and in the character of their appropriate nerves, ganglia and sense organs; that the arachnids possessed skeletal structures comparable, respectively, with the dermal bones, cranium, gill-bars, and notochord of vertebrates; and finally it was seen that the development of the embryo and the formation of the germ layers in the arachnids not only harmonized with, but illuminated the corresponding conditions in the vertebrates."

Now it was hardly possible that vertebrates could by any route come from modern air-breathing scorpions or spiders for the lowest vertebrates undoubtedly came from marine animals. Modern land arachnids were manifestly descendants of a large group of very ancient marine arthropods, the trilobites and merostomes, or giant sea-scorpions, which flourished in the remote Cambrian period, long before any vertebrates were known to exist. In greatly reduced numbers they continued through the following two geological periods, and occurred frequently in the very same deposits in which the first vertebrates appeared.

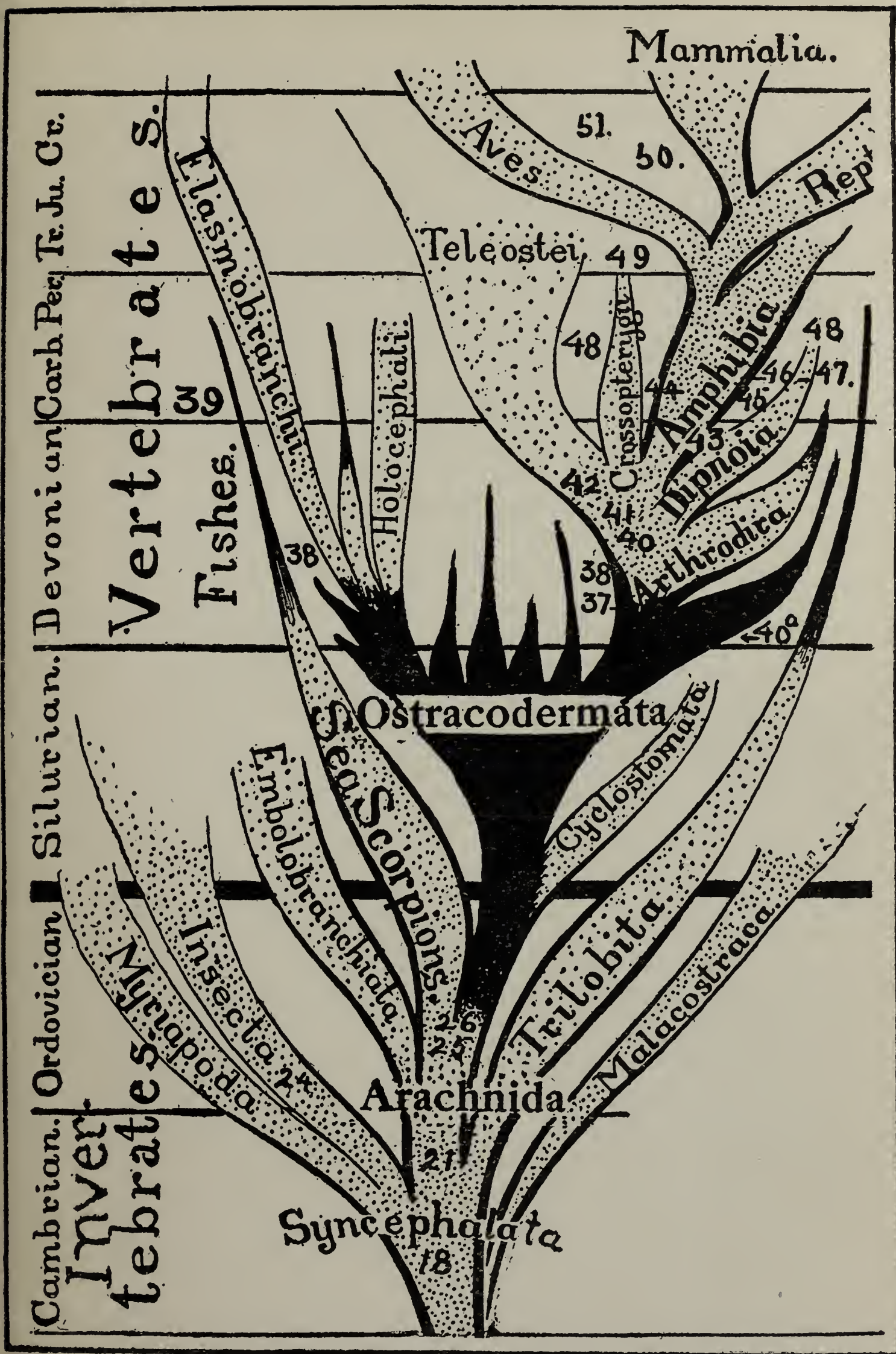
At this point of the inquiry Paleontology stepped in to the rescue of Biology. In intimate association with the declining arachnids

and the expanding early vertebrates there lived in Siluric and Devonian times a strange group called ostracoderms, about which geologists knew little and biologists nothing. In general aspect they resembled somewhat their neighbors the old sea-scorpions, but relations were doubtful in the extreme. By some anatomists they were classed with the invertebrates; by others with the vertebrates, even Huxley and Lancaster concluding that they were very specialized fishes.

But the old antiquated Ostracoderms were not allowed rest. Later examination suggested that they were not true fishes at all, but a new and distinct class midway between the fishes and the marine scorpions. As such they fully explained the resemblance which was known to exist between living arthropods and the vertebrates. They proved, indeed, to be the ancestral form which developed into true fishes and were descendants from the old Cambrian Merostomes. This conception established, all the known facts of anatomy, embryology and paleontology were soon brought into full accord. The ancient sea-scorpions, the ostracoderms, and the fishes really represented three successive stages in the evolution of the vertebrates. Theoretical bridging of the once vast chasm between invertebrate and vertebrate was accomplished.

Nor was fuller testimony of the rocks not forthcoming. With infinite patience and perseverance Patten left his biological laboratories and turned for the nonce paleontologist. In the Devonian rocks of Canada he found in a locality whence some of the Ostracoderms had been described, an abundance of exceptionally illuminating material. Not only were the external details of the forms well preserved but the general character and location of many of the principal internal organs made out. With the theoretical premises the facts as now disclosed fully agreed. This was the most important morphological triumph of the century. So, in the humble Devonian ostracoderm Life made its greatest stride toward that intellectuality which in eons long after was to characterize a reasoning being, and determine the *raison d'être* of an infinite universe.

Use of fire is so exclusively the prerogative of the human family that man may be said to be distinguished from all other animals by the one feature that he alone is a fire-using being. Not only is he the only animal which either kindles or uses fire,



TRANSMUTATION OF THE VERTEBRATES.—(After Patten)

but it is scarcely possible to imagine his being able to exist without it, so large a function does it play in his social and industrial activities.

No tribe of mankind is known which is not acquainted with the use of fire. From time beyond history man is recorded as using fire with which to cook his food and minister to his comfort against the exigencies of the elements. It is not probable that the unfathomable depths of prehistoric antiquity will ever reveal how first man discovered fire, when he first came to enjoy its warmth, or when he first learned to subdue it to serve his needs.

No matter how remote in the distant past there is no period from which traces of human existence have been obtained which does not show evidence of the acquaintance of man with fire. In the oldest archaeological sites of Europe, along with the remains of the cave-man, there are found flints that have been cracked by fire, fragments of charcoal, and charred bones which have been split for the marrow. Yet it must be supposed that there was a time when the primeval human family knew not fire. This is indeed confirmed by the traditions of the ancients, Egyptians, Phoenicians, Persians, Greeks, Chinese and many other nations alike.

No race of savages appears really to have been found so degraded as to be entirely without fire. Most savage tribes have devised some form of fire-making apparatus, notwithstanding the fact that some are so low in the scale that their mastery of fire has not gone beyond the finding of means to keep the smouldering spark alive. And there are a few tribes which cannot relight their fires when once they are extinguished.

That the early employment of fire was accounted marvelous is manifest from the host of myths and legendary traditions which are found among almost every people, regarding the way in which it was either bestowed upon man by the gods, or wrestled from its jealous guardian dieties by the heroic deed or cunning strategy of some glorified ancestor. The numerous forms of fire-worship prove how important was the conquest of fire to primitive civilizations.

Through the use of fire winter is turned into summer, night is converted into day, and man's power of doing it immeasurably multiplied. Judged by its fruits as well as by its broadening

span human life itself is greatly prolonged. By cooking his food man unwittingly destroys these pathologic germs which always before severely fixed his destiny, curtailed his natural life and engendered most of the ills to which his body was heir. Oh, that we today might erect a fitting monument to that unnamed genius who first discovered fire, but whose entity is now so long lost in the gulf of time. By proper cooking of his food and proper care of all his organs it was the great Mitchnikoff's contention that the normal life span of man is not less than 150 years.

At any rate, by cooking his food and thereby removing an unseen, insidious and deadly enemy man is the first animal since original creation, 500,000,000 years ago, to lift itself out of its environment so severely limited by nature, eradicate disease, and by the independence thus gained to defer his old age and increase greatly all the days of his life. It is a discovery of vast possibilities. It is surely one of the great discoveries of life, one of the crises which is bound to have untold effect on the whole future course of organic evolution. It is probably the direct means by which all other forms of life will at no distant day succumb to man. Then will he be the sole survival of the fittest simply because he chanced to survive.

Man thus becomes the first animal in its own life-time to add to his natural acquirements of knowledge all the experiences of his ancestors, a strange and infinitely progressive phenomenon which lately Korzybski expresses belief to be the essential nature of man.

REGENCY OF ORIGIN OF THE ANDES

BY EDWARD W. BERRY

Geologists in general, and particularly the authors of textbooks and speculators on geophysics, assume that periods of earth's crustal folding in the past were contemporaneous with periods of mountain-making and were both but different expressions of the same forces.¹ I am not aware that there is any direct evidence for this assumption, and although I am not proposing to discuss this problem in the present brief outline, I wish to call attention to this feature in connection with the following notes on the age of the Andes.

The subject may be introduced by a brief statement of what is embraced under the term Andes, emphasizing how slight is our detailed knowledge of the age of the rocks in the various ranges; after which effort will be made to piece together the scattered bits of evidence relating to folding and uplift.

The Andes comprise a rather complex upland of ridges and plateaus extending from the Caribbean Sea to Cape Horn, or through some 65 degrees of latitude. As a major feature of the earth's crust their extent is much greater than this, since the tectonic zone of which the Cordillera is the above-sea expression, can probably be closely connected with the Antillean arc on the north, and with the corresponding arc to the southward which connects with Graham Land and runs for an unknown distance across the face of the Antarctic continent. No other mountain system is so continuously lofty, so constantly overhangs a coast, or drops immediately to such profound oceanic depths. The gradient of the south slopes of the Himalayas is similar, but there the lower slopes are covered with the deposits of the Gangetic plain instead of by the waters of the largest ocean. Several

¹ This subject is discussed in some detail by Prof. H. F. Reid, in a paper read before the Geological Society of America, in December, 1920, in which he especially emphasizes the distinction between these two groups of forces.

years ago I pointed out the surprising similarity in the geologic history of this whole region from the Isthmus of Panama to Graham Land.²

So far as I have observed the Andes do not exhibit the Alpine type of structure, with recumbent folds and notable overthrusts, and this is in accordance with the observations of other students. I did not see overthrusting except at one locality near Potosi, Bolivia, and the movements of elevation and depression appear to have been mainly vertical, rather than the result of tangential forces.

In a general way the Andean region can be divided into an Eastern Range and a Western Range, with a series of high valleys, or plateaus, lying between, this arrangement attaining its most typical development in southern Peru and in Bolivia.

In Colombia the Andean system consists of three well-defined ranges, separated by valleys deep enough to be in the Tropical faunal and floral zone. These are the Eastern, Central, and Western Andes, separated by the Magdalena and Cauca valleys respectively. The Magdalena valley is nowhere less than 30 miles wide and suggests a garden. The Cauca valley is 20 to 30 miles wide, and the Central and Western Andes which bound it are directly continued southward to form the Ecuador segment of the system. The Eastern Andes of Colombia appear to represent an additional unit trending more to the East of North and constitute the Cordillera de Bogota, which, in latitude $6^{\circ} 30'$ North and longitude 73° West, forms a knot from which a northern branch dies out west of Lake Maracaibo, and an easternly branch, the Cordillera de Merida, which forms the watershed of Venezuela and which probably connects with the Antillean arc. Its rocks appear to be metamorphic, Cretacic and Tertiary sediments, and Tertiary intrusives. In Colombia the Western Andes do not reach the snow-line, but have no passes below 4900 feet. The Central Andes average about 4000 feet higher, have several snow-clad peaks, and no passes below 10,000 feet. The Eastern Andes have several snow-clad peaks. The Central and Western Andes so far as known consist of ancient igneous and metamorphic rocks and Mesozoic and Tertiary sediments.

The Baudo Mountains, or Coast Range of Colombia, forming the divide between the Pacific and the valley of the Rio Atrato

² Bull. Geol. Soc. America, Vol XXIX, pp. 637-648, 1918.

are unknown geologically; they reach altitudes of about 5,000 feet and some writers consider them a part of the Andean system. Faunal and floral evidences suggest that they were formerly (*i. e.* in post-Miocene time) higher, and formed a link between the Andes and the mountains of Darien. The three Colombian chains converge toward the Ecuador frontier and form the so-called knot of Imbabura, and from here to the so-called Loja knot on the Peruvian frontier the Ecuadorian Andes form a double chain (Oriental and Occidental) trending, rather closely to each other, south by west, and forming a single system united by transverse paramos which divide the inter-Andine area into the basins of Quito (9,500 feet), Ambato (8,500 feet), Cuenca (7,800 feet) and Loja (7,200 feet).

The peaks reach well above the snow-line, about 16,000 feet. Many of them are volcanic. None of the enclosed basins are low enough to be in the subtropical faunal or floral zones. Ecuador also has a Coast Range trending northwesterly from Guayaquil which is reported to be composed of Mesozoic sediments and porphyritic eruptions like those of the Northwestern Range, of which it may be a spur. Little is yet known of the geology of the Ecuadorian ranges. I should expect it to be similar to that of the Colombian ranges.

The tectonics around the Loja basin are but little understood. The Rio Zamora which drains it into the Marañon lies in a deep valley, south of which the Western Andes, or Maritime Cordillera continue southward, west of the valley of the Marañon (the Cordillera Negra of Peruvian authors). East of the Marañon are the more lofty snow-clad Cordillera Blanca, which extend from the great bend of the Marañon southward to where they join the main western chain at the knot of Cerro de Pasco, in latitude $10^{\circ} 30'$ south. Farther to the east, in northern Peru, and forming the divide between the Huallaga and Ucayali, is another range, little known and variously correlated. From the knot of Cerro de Pasco south to the knot of Vilcanoto, or Cuzco (Lat. $14^{\circ} 30'$), the system has a breadth of about 250 miles and the ranges are sometimes grouped as Western, Central and Eastern. All but the Western, in which the passes are two or three thousand feet higher than in the more Eastern Range, are illy defined and much broken with narrow, high valleys and punos.

In southern Peru the Western, or Maritime, Cordillera is well

developed and it extends from here southward in an unbroken mass from the knot of Vilcanota to Cape Horn. Lying east of the Western Range, and extending from Vilcanota almost to latitude 22° south, is the Titicaca-Poopo basin, or the altaplanicie of Bolivia. This is bounded on the east by the Cordillera Oriental of southern Peru (Los Andes of Paz Soldan), and these continue southward, as the Cordillera Real of Bolivia, fanning out south of Tres Cruces to form the broken country constituting the Departments of Cochabamba and western Chuquisaca, and extending eastward as far as Santa Cruz de la Sierra, nearly 500 miles from the Pacific coast. To the east they gradually decline and disappear beneath the Amazon and Paraguay plains. To the south they decline and virtually disappear in southern Bolivia. Peru has a low Coast Range in the northern and southern parts of the state largely made up of granitic rocks (diorite and granodiorite).

In southern Peru and Bolivia the Eastern Andes consist largely of Siluric and Devonian shales. They contain some marine Carbonic beds and shallow-water Late Cretaceous deposits as well as Pliocene continental deposits, together with Pliocene granitic batholiths and porphyritic rocks. In their eastern border region they contain some Ordovician formations, and toward Santa Cruz a northward continuation of the Permian tillites of Argentina. The rocks of the Altaplanicie are Devonian, Carbonic, Cretaceous, and Pliocene to Recent in age. The Western Range, so far as known, consists entirely of Mesozoic sediments and very late Tertiary volcanics, and they retain this character southward to the Antarctic, but with some continental Rhaetic beds toward the south.

Chile has one Andean system (Los Andes), a direct continuation of the Maritime, or Cordillera Occidental of Peru and Bolivia. It is a single, broad, montane zone, the watershed of which to the southward has been shifted eastward by glacial action, as noted independently by P. Quensel³ and B. Willis⁴, with the consequence that several rivers pass westward in canons amid the high peaks along the crest, and flow into the Pacific. From about latitude 30° to the Tropic of Capricorn Los Andes are said to show a zone of faulting separating them from the graben lying

³ On the Influence of the Ice Age on the Continental Watershed of Patagonia: Bull. Geol. Inst. Upsala, Vol. IX, 1910.

⁴ Physiography of the Cordillera de los Andes between latitudes 39° and 44° South: Comptes rendus, XII Cong. Geol. Int. pp. 733-756, 1914.

between Los Andes and the pre-Cordillera of Argentina, the latter believed to be an area of old Paleozoic and pre-Cambrian rocks, which were folded in pre-Siluric time, according to H. Keidel.⁵ West of the Andes, in Chile, there extends the great longitudinal valley of Chile which has the appearance of a great graben, and which is submerged south of Puerto Montt, in latitude 42°. The only other Chilean mountains are the relatively low Cordillera de la Costa, partially submerged south of latitude 42°. They are reported to consist of ancient crystalline rocks, but contain metamorphosed Mesozoic sediments near Concepcion, which throws doubt on their great age.

The questions of pre-Cambrian history and the age of the crystalline rocks are seen to be one of great indefiniteness, when the Pliocene age of the granite batholiths and associated porphyritic rocks of the Eastern Range, and the Cenozoic age of the intrusive rocks of the Western Range, both of which are established facts⁶, are considered.

Some students have considered that the Andean geosyncline was compressed between the Brazilian and Argentine massifs on the East, and a Pacific massif on the west of which the sole existing remnant is that furnished by the crystalline rocks of the low coast ranges of Colombia, Peru and Chile. There are many reasons for thinking this to be probable; but it should be recalled that some of the crystallines in the Bio Bio valley of southern Chile are either metamorphosed Mesozoic marine sediments or are so intimately associated with the latter that doubt is thrown on the age of the whole.

That there was a Pacific land-mass to the west, which persisted, to some extent at least, as late as Miocene time, is supported by the following considerations:

- (1.) There is full analogy from other regions, *e. g.*, Appalachia.
- (2.) A land area over the site of the Andes is indicated by the absence in most of the region of pre-Silurian deposits.

Late Ordovician (Arenig) graptolites were collected by Evans

⁵ Über das Alter, die Verbreitung, und die gegenseitigen Beziehungen der verschiedenen tektonischen Strukturen in den argentinischen Gebirgen: *Comp. rendu*, XII, *Cong. Geol. Int.*, pp. 671-687, 1914.

⁶ *Proc. U. S. Natl. Mus.*, Vol. LV, pp. 279-294, 1919; also, J. T. Singewald; *Economic Geology*, Vol. XVI, pp. 60-69, 1921.

in the Caupolican district of Bolivia ⁷. Steinmann ⁸ records Early Paleozoic strata in southeastern Bolivia. Keidel ⁹ considers the pre-Cordillera of northwest Argentina as having a basement of rock folded in pre-Cambrian times.

(3.) Evidences indicate that the marine Late Carbonic transgression came from the east, and that the plant-bearing Carbonic beds of Paracas peninsula, in about latitude 14° S., on the Peruvian coast, were littoral in character and on the eastern margin of an ancient land-mass.

(4.) There is total dissimilarity between the early Miocene fauna of northern Peru and southern Chile, showing an entire lack of communication between the two regions at that time, and an arrangement of the land which prevented the prototype of the present Humbolt current from reaching Peru.

(5.) There is complete absence of marine Eocene, Oligocene and Miocene deposits between latitudes 6° and 34° S.

(6.) By the presence, between these latitudes, of a deep longitudinal trough (Peruvian and Chilean deeps) immediately off the present coast the consequent gradient being one of the steepest in the world, and indicating a fault-scarp. This interpretation is emphasized by the great seismic and volcanic band of the Western Andes, and the recency of this faulting is indicated by the fact that adjustments throughout the whole length of the Western Andes are still actively taking place. Earth tremors are constant and the numerous disastrous quakes are a matter of human history.

The next item of Andean history is the transgression of the Siluric and Devonian sediments which form so considerable a part of the rocks of the Eastern Andes in Peru and Bolivia, and whose folded hog-backs gradually disappear beneath the detrital deposits that cover the altaplanicie of Bolivia.

Late Devonian and Early Carbonic fossils have not been discovered in the Andes and there is some evidence that marked folding and emergence occurred at this time, in the discordance between Devonian and Late Carbonic (Ouralian) observed by H. Gerth ¹⁰ in the Eastern Andes.

Next followed transgression of Late Carbonic rocks correlated

⁷ Quart. Jour. Geol. Soc. London, Vol. LXII, p. 425, 1906.

⁸ Neues Jahrb., Beil. Bd. 34, pp. 176-252, 1912.

⁹ Op. cit.

¹⁰ Geol. Rundschau, Bd. 6, pp. 129-153, 1915.

with the Ouralian stage of Europe, with a subsequent emergence lasting through the Permian and Triassic times. There is a northward extension of the Permian tillites of Argentina into Bolivia in the Santa Cruz region. In Late Triassic (Noric) times a marine invasion may have reached Peru and Colombia from the Upper Amazon region and there are some continental Rhaetic deposits in Chile.

Then followed a Jurassic submergence of unknown limits but general throughout the Western Range region. According to J. A. Douglas¹¹ this was followed by post-Jurassic uplift and batholithic intrusion. The latter is probably of a still later age, and it is questioned whether there was really any great amount of uplift. That there was shallowing in many areas is shown by the littoral character of the latest Jurassic beds and the widespread Early Cretacic coals of Peru. I believe that there was much minor oscillation of the strand throughout Cretacic times, but the details are still obscure. Traces of coal-beds, or shallow marine faunas, occur in the Andes from the Caribbean Sea to Cape Horn. They are especially prominent in the Western Range throughout its whole extent; and are known on the Altiplanicie and in the Eastern Range of Bolivia, and they are probably present elsewhere.

The Andean region evidently has been a land area, throughout most of its extent, at least, even since the Late Cretacic times, but that the major folding of the post-Cretacic strata was not contemporaneous with the major uplift that formed the present Andes is indicated by such facts as the following: The Miocene flora, found in the Tumbes-Payta region of coastal Peru, is identical with that of the Amazon basin so that there could have been no mountain barrier at that time sufficient to differentiate these two districts, or the fossil flora of the same age found to the west of the present Western Andes in the Concepcion-Arauco district of Chile. At the present time, in northern Peru, the upper limit of the tropical zone is at 4500 feet and, according to local topographic conditions, it sometimes reaches an altitude of 6,000 feet. From this it seems that until after early Miocene times there were no mountains in that region with a divide as high as 6,000 feet. Moreover, in the Eocene epoch there were marine transgressions from both the Atlantic (San Jorge formation of Argentina) and

¹¹ Quart. Jour. Geol. Soc. London, Vol LXXVI, No. 301, 1920.

from the Pacific (Negritos and Lobitos formations of Peru, and unnamed formations in Colombia) on the flanks of the Andean land-mass. Similar marine transgressions of Early and Mid Miocene times are recorded in the Zorritos and Talara formations of Peru and Ecuador, the Navidad beds of Chile, the Patagonian beds of Argentina, and unnamed formations in Colombia and Venezuela. There is no paleontologic evidence for the remainder of the Miocene period but there is a considerable amount of physiographic evidence for predicating a considerable uplift during the late Miocene, and possibly also the early Pliocene times, with great volcanic activity, the formation of lava-plateaus, and a subsequent mature erosion of the upland with valley filling. Every competent observer who has visited the Andes has been impressed with the mature topography of the Andes in the cycle of erosion that preceded the most recent one and the only differences of opinion have been as to the relative dates at which these events took place.

The Williams Expedition, which the Johns Hopkins University sent out in 1919, collected abundant data bearing on this very question, all of which are still unpublished, so that I can only give it in very brief form in the present paper. Extensive fossil floras of unquestionable Pliocene age were found at a number of localities in the Eastern Andes and on the altaplanicie of Bolivia, which show that both these regions were at that time much nearer sea-level than now, and that there were no elevations sufficient to reach the snow-line, nor to interfere with the tradewind circulation. The maximum elevation could not have been over 6,500 feet and it may have been considerably less than that figure. A preliminary account of one of these floras (that from Potosi) I published a few years ago.¹²

Following this stage of widespread lowland mesophytic floras over the now high and arid uplands of Bolivia, differential movements are recorded by slight folding, warping and faulting of these Pliocene deposits, followed by late Pliocene and probably pre-Glacial Pleistocene uplift in the Eastern and Western Ranges, with relative or positive sinking of the inter-Andine plateaus and some at least of the trans-Andine plateaus and valleys, and the extinction of the so-called Pleistocene faunas found in the high Andes

¹² Proc. U. S. Natl. Mus., Vol. LIV, pp. 103-164, 1917.

from Ecuador to Chile, which had invaded the region from the Argentine plains. This circumstance also shows the lack of efficient barriers at that time to prevent dispersal, as does also the presence of vegetation sufficient to sustain the enormous bulk of the ground-sloth and similar giant vegetarians.

This differential movement is shown by the enormous thickness of late Pliocene or early Pleistocene, of many thousand feet of apparent thickness (probably slope deposition) found on the altaplanicie and east of the Front Range in Bolivia. Continued elevation of the mountain segments in the late Pliocene, or early Pleistocene, resulted in the formation of mountain glaciers, canon-cutting and valley-filling.

There has been some elevation of the mountain segments in post-Glacial time, resulting in cutting off the supply of precipitation from peaks which were once snow-clad, and in the very youthful box-cans and valley-gorges of intrenched streams, the ungraded river-beds and the recently dissected mature slopes, *e. g.* east of La Paz.

Summing up the evidence that major uplift of the Andes was post-Miocene in point of time, it may be stated, that,

(1.) There was measurable change in climate, largely due to altitude shown by the Pliocene fossil floras of Jancocata, Corocoro, Potosi, Pisllypampa and other localities in Bolivia, where plants which could not live above 5,000-6,000 feet are found in fossil state at elevations ranging from 11,800 to 13,500 feet.

(2.) There is abundant evidences of the existence of fault-scarps along Pacific Coast, along the Andean Front Range,¹³ and along the Front Range of Bolivia (as observed by Mertie and others), in southern Chile, and between the Andes and the pre-Cordillera in Argentina.

(3.) There are antecedent streams of Peru and Bolivia which flow to the Atlantic ocean, and those of southern Chile which reach the Pacific.

(4.) Cape Fairweather oyster beds of Patagonia, which are at sea-level along the Straits of Magellan, rise to elevations of over 5,000 feet in the Andean spurs of Patagonia, as observed by Hatcher.

(5.) Recent dissection of mature topography and the dis-

¹³ Recorded by I. Bowman.

cordance of from 4,000 to 5,000 feet in the east and west projection of the valley floors in the Peruvian Andes.¹⁴

(6.) Seismic and volcanic evidences seem conclusive.

(7.) Since the biologic evidence is likely to be somewhat unfamiliar to most geologists, some space may be devoted to it. Floral and faunal criteria are available only north of latitude 5° S.

The fresh-water fish fauna of the Guayas basin of Ecuador exhibits so close an affinity with that of Amazon that Scharf¹⁵ concludes any elevation must have been comparatively recent and this is confirmed by Henn¹⁶ and by the more detailed studies of Eigenmann.¹⁷

According to T. Wolf,¹⁸ the flora is like that of the Chocó (littoral of Colombia) and Panama and the Amazon part of Ecuador. Chapman¹⁹ states that the avifauna fully confirms the other biologic evidence, and that this was the latest passage for tropical species to be closed.

The distribution of the existing flora and fauna in Colombia, so far as one can judge, appears to be entirely conditioned on topographic features of so recent a date of origin that sufficient time has not elapsed to permit of any considerable differentiation between those of the upper Orinoco and Amazon basins, the inter-Andean valleys, and the Pacific coastal region. No features can be correlated with any earlier montane topography. Chapman in his very excellent account of the Birds of Colombia (*op. cit.*) concludes that the bird-life of the Pacific Coast of Colombia and Ecuador is largely a pre-Andean fauna which was continuous with that of the Upper Amazon region, until cut off by the Andean uplift; that of the Cauca valley he regards as post-Andean. This uplift was so recent that sufficient time has not elapsed for differentiation on the two sides to have advanced as far or beyond the point where anything more than subspecies can be recognized. Wide ranging forms are disregarded so that the conclusions reached are entirely valid.

¹⁴ In *The Andes of Southern Peru*, 1916, I. Bowman, gives an excellent summary of the physiographic proof of recent great uplift.

¹⁵ *Dist. and Origin of Life in America*, p. 360, 1912.

¹⁶ *Science*, N. S., Vol. XL, p. 603, 1914.

¹⁷ *Indiana University Biological Studies*.

¹⁸ *Geographia y Geologia del Ecuador*, p. 439.

¹⁹ *Bull. American Mus. Hist.*, Vol. XXXVI, p. 110, 1917.

BLISTER HYPOTHESIS OF LACCOLITHIC
MOUNTAINS

BY CHARLES KEYES

INTRODUCTORY

Touching casually upon the question of origin of those anomalous intrusive bodies which we are pleased to designate laccoliths the late James D. Dana once remarked that there were "Various opinions but no positive knowledge." In the generation which has passed since this trite statement was made there is much that is new which has been deciphered concerning the conditions underlying this special form of magmatic activity. Between the two extreme views that have been presented, between the idea of an easily floated prism of strata which develops into a symmetrical, dome-shaped mountain, and the notion that a laccolith is a mechanical impossibility, there is a middle ground which although rendering improbable the one and perfectly invalidating the other is amply supported by wide observation and withal has the further advantage of being mathematically sound. The genetic control thus turns out to be dominantly orogenic rather than simply hydrostatic in nature. The most conspicuous feature involved thus becomes quite secondary instead of distinctly primary in character.¹

Until G. K. Gilbert formally christened (1877) this distinctive type of mountain laccolithic structure had attracted small practical attention. Nevertheless long before the Henry Mountains of Utah were graphically portrayed laccolithic forms were clearly figured out. Under other titles the identic tectonic forms were early known. Although not particularly distinguished from ordinary bosses certain special phases of the latter were differentiated. Fifty years prior to the appearance of the mem-

¹ Characteristics of Volcanoes, p. 24, New York, 1890.

oir in which the term laccolith was first defined Maclaren² described as typical and as perfect laccolithic mountains as any later writer pictures forth. Other Scotch geologists long ago also directed attention to similar structures.

Nor was this country very far behind in recognizing this class of mountains. Long prior to the appearance of the sumptuous monograph on the Henry Mountains typical laccolithic phenomena were, within plain view of that group, clearly pointed out and fully described. Very much in the same manner as subsequently discussed by the author of the monograph similar forms of laccoliths were outlined and their essential characteristics and structures faithfully portrayed. Previous to Gilbert's entering the southern Utah field his identical explanations of laccolithic origin of certain Great Basin mountains was succinctly set forth and repeatedly offered. Under the title of "Eruptive Mountains" was this particular orographic type widely known to many of his predecessors and contemporaries in the region.

In the Utah country, J. S. Newberry, geologist to the Macomb expedition, appears to have been the first investigator to call attention to that especial type of orographic structure which years afterwards came to be designated the laccolithic mountain. So early as 1859 Newberry,³ after visiting the Sierra Abajo, situated 100 miles east of the Henry Mountains, in eastern Utah, accounted for its peculiar structure by regarding it as an eruptive nucleus on which steeply reclined the sedimentaries. He also spent several days on the Sierra La Sal, forty miles to the north, to which was ascribed a similar origin. His descriptions leave no doubts concerning the laccolithic nature of both mountain groups.

Various members of the U. S. Geographical and Geological Surveys of the Territories, clearly describe laccolithic mountains. A. R. Marvine's lucid descriptions⁴ of the Elk group of laccoliths, in western Colorado, are particularly noteworthy. To this singular orographic type F. V. Hayden early applied⁵ the title of "Eruptive Mountains." W. H. Holmes' sketches⁶ of the Elk Mountains display clearly the wedge-shaped masses intrusive in

² Geology of Fife and Lothians, p. 100, 1839.

³ Geology of Macomb Exped., p. 100, 1876.

⁴ Seventh Ann. Rept. U. S. Geog. and Geol. Surv. Terr., for 1873, p. 186, 1874.

⁵ *Ibid.*, Eighth Ann. Rept., for 1874, p. 55, 1876.

⁶ *Ibid.*, Eighth Ann. Rept., for 1874, pp. 62-64.

Cretacic strata, and the bowing of the sedimentaries over the dome formed by the bulging body. In his investigation of the Sierra El Late in the following year Holmes⁷ presented the essential features of laccoliths almost as clearly as did Gilbert those of the Henry Mountains several years later. Peale⁸ further recognized the laccolithic peculiarities of the Sierra La Sal, and especially emphasized the idiosyncracies of formation.⁹

In this connection no special reference is made to the observations of others who describe isolated mountains with eruptive nuclei surrounded by tilted sedimentaries having quaquaversal dips, but who give no intimation of the real structures. Among these, however should be noted the records of F. V. Hayden, in the Geology of Yellowstone Park Expedition, Henry Newton, in the Geology of the Black Hills, G. M. Dawson, in the Geology of the Forty-ninth Parallel, and N. H. Winchell, in the Geology of Ludlow's Reconnaissance of the Black Hills.

When, then, the Geology of the Henry Mountains was published in 1879, without recognition of any previous work along the same lines about all that there was left that was really new was to fix the type by giving it a specific name. Because of this fact, and Gilbert's monographic treatment of a single phenomenon thereby drawing upon him the chief criticism of the hypothesis of origin his predecessors in the field were lost sight of. In view of later developments the tectonics of the Henry Mountains have to be examined anew with normal orogenic conditions in mind.

The basis of the accompanying notes is a collateral result of some recent mine inspections. Chance diverted the inquiry from the questions of ore deposition after the latter had been disposed of to the broader geologic problem of association of the laccolithic intrusions with the tectonic features. Later other laccolithic fields were visited. Opportunity did not present itself to review the Henry Mountains on the ground and in the light of the recent tectonic results; but enough is gathered from the published accounts and from inspection of other southern Utah laccoliths to indicate clearly that some of the more notable of the ascribed features must be interpreted anew.

⁷ *Ibid.*, Ninth Ann. Rept., for 1875, p. 268, 1896.

⁸ *Ibid.*, Ninth Ann. Rept., for 1875, p. 95, 1876.

⁹ *Ibid.*, Bull. No. 3, p. 557, 1877.

TYPE-FORM OF LACCOLITHS

Ideal Cross-section of Henry Mountains. According to Gilbert¹⁰ laccoliths, sheets and dikes are genetically one. "Between the sheet and laccolith there is complete gradation. The laccolith is a greatly thickened sheet and the sheet is a broad, thin, attenuated laccolith." The familiar diagrammatic representation of a laccolith (figure 1) indicates a simple bulge of strata by out-



Fig. 1. Ideal Form of Laccolith. (After Gilbert.)

wardly forced magma which from some cause or other does not, as in normal volcanic eruptions, reach the surface of the ground, but insinuates itself between rock beds at an horizon some distance from sky.

It is the mechanical possibility such as the ideal conception which Gilbert conveys that is so sharply challenged by European geologists, notably Suess,¹¹ Neumayer,¹² Reyer,¹³ and Geikie.¹⁴ Still other features seem yet quite inexplicable and still are questioned by many American investigators. In view of Neumayer's categorical statement that the Henry Mountains intrusion as explained by Gilbert "borders almost on the impossible and incredible" the query arises as to what conditions, if any, exist by which laccolithic intrusion becomes a potential mechanic reality.

In the light of subsequent and more complete observation the simple blister form of magmatic body probably never does actually obtain. As a geological phenomenon it now seems hardly more

¹⁰ *Geology of the Henry Mountains*, p. 20, 1879.

¹¹ *Das Antlitz der Erde*, I Bd., p. 195, 1885.

¹² *Erdgeschichte*, I Bd., p. 180, 1887.

¹³ *Theoretische Geologie*, p. 135, 1888.

¹⁴ *Text-book of Geology*, 4th Ed., Vol. II, p. 736, 1903.

than a figment of the imagination. From what is lately gleaned from a consideration of the Henry Mountains region there appears to be without doubt certain larger tectonic associations with the smaller laccolithic structures of which no mention is made. Although the ideal cross-section of the Henry Mountains may prevail in one direction in all but exceptional instances it seems never to occur in all directions in the same laccolith. In all likelihood it is the stiffness of a diagram of the ideal conception that has done more than anything else to delay the ready and universal acceptance of the laccolith as a specialized expression of magmatic intrusion. Along with the discernment of well defined tectonic affinities of laccolithic structure the type or ideal form assumes outlines wholly different from that originally suggested. With the new theoretical conception the observed facts seem strictly to agree.

Normal Asymmetry of Laccolithic Masses. In his graphic account of that laccolithic group in western Colorado, known as the West Elk Mountains, Dr. Whitman Cross¹⁵ incidentally shows in a diagram that one of the principal elevations, Mount Marcelina, is, in cross-section, essentially a thick wedge-shaped mass instead of a symmetrically developed lenticular body. It does not seem to have dawned upon this writer that this might possibly be the normal shape of laccolithic bodies rather than the exceptional form. The same is true of the neighboring Anthracite Mountain. In the course of his theoretical discussion of the forms of laccoliths Cross indicates that in the case of the irregular masses faulting of the strata once underlying might take place, and if the rock column were very thick the break might pass upward into a monoclinal fold.

In New Mexico all members of the Sierra del Oro group of laccoliths appear to be notably asymmetrical in shape. Careful inspection of other laccolithic mountains clearly discloses the fact that they too are not regularly lenticular in form as once supposed, but are really irregular or rather cuneiform in transverse outlines. The Tuertos Mountains of New Mexico for example, present the following structural aspects as represented in cross-section (figure 2). This diagram may be with full assurance taken as the ideal or type form of all laccolithic intrusions.

¹⁵ Fourteenth Ann. Rept. U. S. Geol. Surv., pt. ii, p. 184, 1894.

The restored profile of Mount Marcellina, of the West Elk Range, modified after Cross displays the same essential characteristics (figure 3).

Even the Henry Mountains now seem to come under the category of the asymmetrical type rather than the one originally

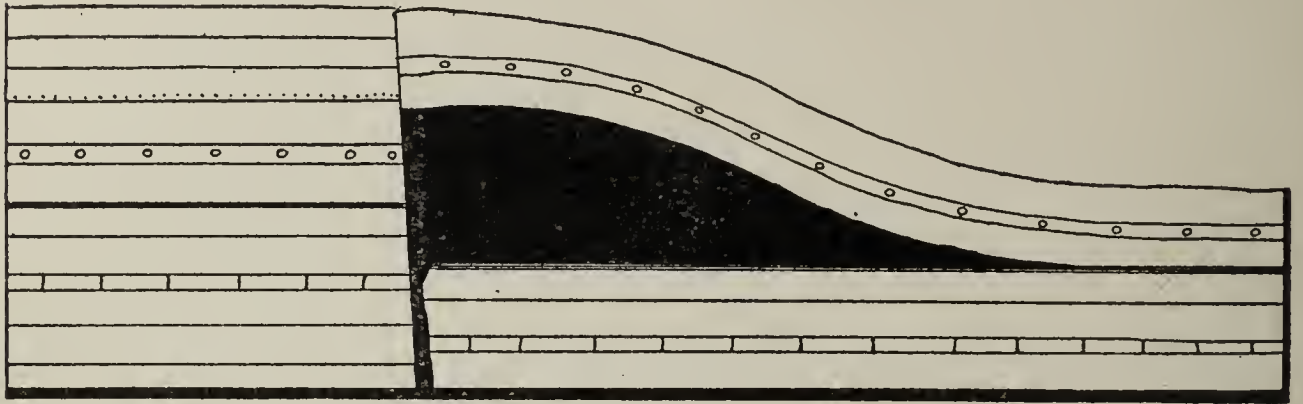


Fig. 2. Cross-section of Typical Laccolith.

formulated for them. The diagrammatic cross-section of Mount Hillers is plainly asymmetric rather than regular, as Gilbert's theoretical ideal calls for, indicating that perhaps the actual symmetry is not so perfect as is argued, and that the impressions gained from original observations subconsciously did not coincide entirely with the assumed demands. There are besides many observations recorded in the Henry Mountains monograph of



Fig. 3. Cuneiform Laccolithic Structure.—M. Marcellina, Colo.

which no use is made beyond mere mention of the facts. Others there are which require additional interpretation. Still others indicate that had they been interpreted less hurriedly there might have resulted a very much modified generalized ideal.

The type-form of the laccolith is, therefore, to be regarded as a cuneiform, or wedge-shaped, body which is a direct expression of its genesis.

Seeming Incongruity of the Bysmalith. A third structural aspect of dome mountains is the one designated by Prof. J. P. Iddings¹⁶ as "Plug" mountains, or Bysmaliths, with Mount Holmes at the southern end of the Gallatin Range, in Yellowstone National Park, as the type. The theoretical conception of this structure is graphically represented below (figure 4):

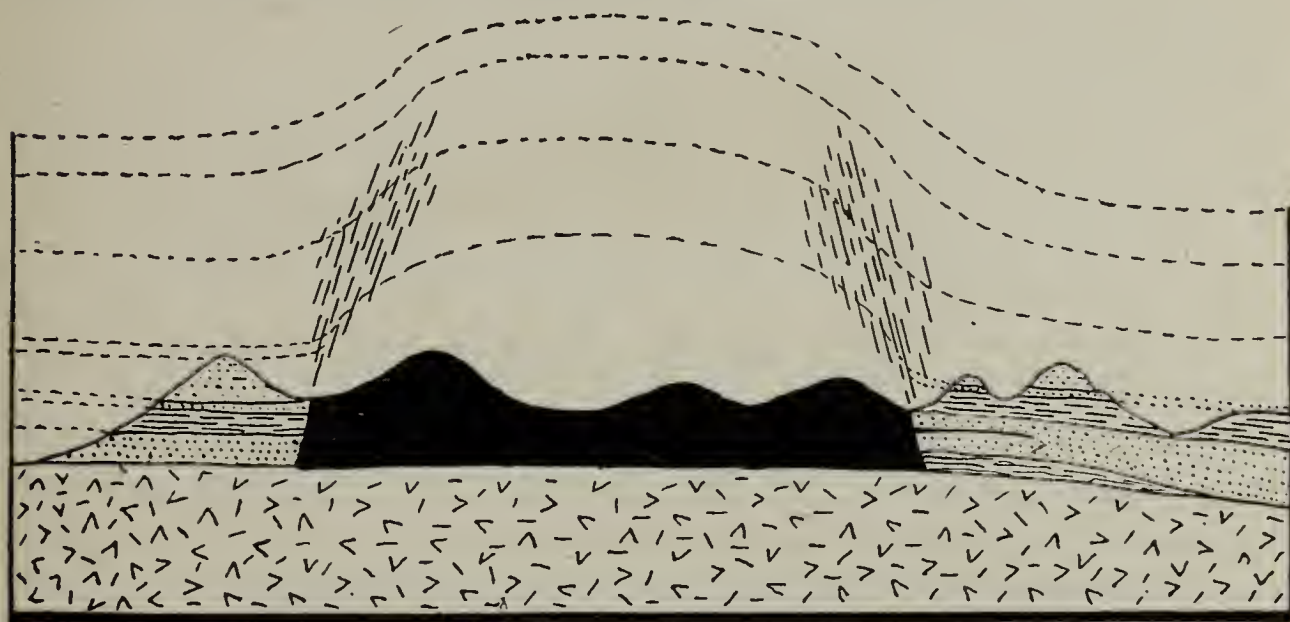


Fig. 4. Type-form of Bysmalith. (After Iddings.)

Neither in the preliminary description, nor in the more mature account¹⁷ are the necessary details given to prove that this form of bulging intrusion is to be properly classed with laccoliths rather than with volcanic plugs. More recent visits to the Yellowstone region discloses facts that point to the essential structures of Holmes Mountain as not referable to intrusive phenomena but to diastrophic movement. However, under present consideration these circumstances are not pertinent. The main thing to be noted is the fact that profound faulting is associated with the phenomenon. Its direct bearings upon typical laccolithic intrusion is referred to at length elsewhere.

Until recently the conception of a bysmalith existed only as a hypothetical possibility. Only with the disclosure of the Ortiz structures does it become an object of actual observation. Insofar as it relates to a special modification of the laccolith is Iddings' definition a valid one. Professor Hobbs' attempt¹⁸ to widen the meaning of the term imparts a totally distinct significance, and

¹⁶ Journal of Geology, Vol. VI, p. 704, 1898.

¹⁷ Mon. U. S. Geol. Surv., Vol. XXX.

¹⁸ Earth Features and their Meaning, p. 442, New York, 1912.

one which is exactly synonomous with the name laccolith itself as now understood. Of course Iddings' original diagram of a bysmalith leaves the impression of what Harker¹⁹ afterwards designated a "plutonic plug."

As first supposed to exist the bysmalith was regarded as one extreme of laccolithic development in which the vertical axis of the intrusive body was greatly extended; on the other hand the extreme development of the horizontal axis was considered as producing the sill or sheet as urged by Gilbert. The necessary consequences of the first assumption was to associate the laccolith with the extrusive, or volcanic, rather than the intrusive phases of magmatic outwelling. This tendency is shown in many if not the majority of the allusions to laccoliths. On the excursions of the Seventh International Geological Congress through the Caucasus region K. Roguéwitch²⁰ and others among the Russian geologists, pointed out the eruptive masses of Piatigorsk as European types of the American laccoliths. More recently Van de Derwies²¹ takes the same view. These hills, however, are manifestly merely old denuded volcanic necks²² and find their nearest American affinities in the El Cabazon field of the Mount Taylor district of New Mexico.

Relations between Bathyliths and Laccoliths. The common conception of a bathylith is a laccolithic blister of giant proportions. Unlike the laccolith the bathylith is fancied as having no determinable floor, which later, according to Suess²³ may be entirely fused and mingled with the magma. As the central core of great mountain chains the granitic body is so large that the accompanying faulting of the overlying strata is relatively almost infinitesimal and the stratified formations form essentially true arches. To these circumstances the Gilbert notion of an ideal, regular laccolith exactly fits. As expressions of similar orographic powers laccoliths and bathyliths are clearly and closely related genetically. Because of a vast difference in size local displacement of strata largely determines the one while it has little or nothing to do with the other. On account of circumstances

¹⁹ Nat. Hist. of Igneous Rocks, 1909.

²⁰ Guide des Exc. du VII Cong. géol. international, XVII, p. 5, 1897.

²¹ Rech. géol. et pétrog. sur les laccolithes des environs des Piatigorsk, 1905.

²² Proc. Iowa Acad. Sci., Vol. VII, p. 137, 1901.

²³ Sitzunberichte der Weiner Academie, Bd. CIV, p. 52, 1895.

of this kind such classifications of intrusive rocks as that recently proposed by Professor Daly utterly fail in the genetic element. Bathyliths, laccoliths and bysmaliths are placed in distant categories fundamentally removed from one another whereas genetically they constitute a single order. It is notorious that the basal plane of laccoliths crosses the bedding planes of the intruded strata as often as it coincides with them. As shown in the Ortiz intrusive a bysmalith is about as far removed from a volcanic neck as it is possible to have it.

Formative Relations of Laccoliths. As laccolithic masses come to be more and more carefully scrutinized theoretical considerations begin to point more and more to the circumstance that immediately prior to intrusion distinctive tectonic conditions and especial stratigraphic structures must constitute more important determining causes. What appears to be usually mistaken for evidence of hydrostatic pressure of the magma is in reality orographic potentiality of the same category as flexing in mountain building. The magma itself seems to play merely a passive rôle. Perusal of the literature on laccolithic themes, published during the past 30 years, since the date when the term was first fixed as applying to a distinctive conception likewise shows that with constantly increasing frequency reference is made to displacement as an accompaniment of this particular type of intrusion.

Recent critical examination of the broader structural features of some of the New Mexican laccolithic mountains seems to connect directly the intrusion of the eruptive masses with not only contemporaneous faulting but with both more anciently acquired regional tectonics and profound displacement dating some little time prior to the main accumulation of magmatic materials.

A number of facts militates strongly against the Henry Mountains explanation of laccolithic protuberance. Three basic premises appear wholly untenable. Most vitiating is the seeming incompetency of simple hydrostatic pressure to produce the desired results. Inadequacy of relative lithologic density is now commonly conceded. There also appears to be a radical disparity between the physical conditions accompanying the formation of laccoliths and their once supposed nearest kin the sills.

On the other hand the recent unearthing of the infrabasal make-up of certain laccoliths clearly points to a fundamental

dependence of this class of mountains upon prior geologic structure. The shape of laccolithic masses is found to be cuneiform instead of lenticular; and thus at once does away with the blister idea. Quite essential appears to be the presence of crustal lines of weakness. The magmatic swelling or localization of laccoliths is discovered to be a direct function of orographic potentialities.

In seeking an immediate cause for this laccolithic intrusion Gilbert did not lose sight of certain mechanical shortcomings of his explanation. These he sought to overcome by appealing to certain associated factors, which, however, later, Whitman Cross showed to be both unnecessary and not demonstrative as such. Dana got over the difficulties by brushing aside all considerations except simple hydrostatic pressure and with this feature alone regarded the Gilbertian hypothesis complete. This is doubtless one of the main reasons why from a mechanical angle leading European Geologists have so persistently challenged the American view of laccolithic intrusion. At the same time Old World writers on the theme offer no alternative theory to take the place of the one which they seek to discredit. Through the results of close inspection of certain laccoliths of northern New Mexico the chief objections which were raised against the Gilbert view seem to be fully met. Controlling tectonic factors which all describers of laccoliths have missed thus appear to supply the long sought desiderata.

As a primary consideration in order that a laccolith be produced rather than any other form of volcanic manifestation it appears that the intrusive mass shall have a particular tectonic setting. Profound faulting is one of these prime factors. Another is orographic flexing by which the rigidity of certain arching strata largely maintains the load of superincumbent materials. Probably the high viscosity of acidic magmas has an important but as yet uncalculated influence on events. The remarkable infrabasal structure which the New Mexico laccoliths reveal carries the inquiry a step more remote and explains the deep-seated cause of the major faulting, whereby an orographic prism is sustained by a sharp Pre-Cambrian arch, the rigidity of which is not even yet lost although the adjoining blocks on either side are allowed to slide down, as it were, the steep sides of the old flexure.

UPPER PALÆOZOIC FAUNAS OF MISSOURI ¹

BY HENRY S. WILLIAMS

During the years 1889 and 1890, fossils have been collected by Mr. W. P. Jenney, Mr. James D. Robertson and Mr. Gilbert Van Ingen from numerous localities in the western half of Missouri, south of the Missouri river, and have been sent to me for study. I have made preliminary examination of them; and I have also had collections from northern Arkansas and other adjoining states for comparison.

During the month of August, 1890, I made a hasty examination of the strata around the south-western border of the Ozark uplift, from Batesville to Eureka Springs, Arkansas, and thence across Missouri as far as to Sedalia.

At the present time I am able to present a few general facts resulting from these studies, leaving, however, the details until more thorough investigation shall be made.

I have indicated in the classification proposed the positions in the geologic column where, upon palæontologic grounds, the sharper lines of delimitation are to be drawn.

The formations about the southern and western margin of the Ozark uplift are sharply divided into two terranes, one, Lower Palæozoic, and the other, Upper Palæozoic.

(A.) The Silurian formations, which for most of the region are Lower Silurian, rarely reach higher than the lower formation of the Upper Silurian.

¹ This article was originally prepared by Professor Williams for Bulletin No. 3, of the Missouri Geological Survey, published in 1890. After it was set up in type and in page form there arose some differences of opinion regarding terminology and Mr. Winslow, then State Geologist, at the last moment refused to publish it, and it was withdrawn. Since this is Professor Williams' first outline of his proposed changes in the grouping of the Early Carbonic terranes of the Mississippi Valley, after his review of the life zones, and as many of the points therein illuminated are little considered by later writers on the subject its publication, even after an elapse of 30 years, seems worth while in order to clarify somewhat these moot points. As will be noted, the paper discusses a number of features not mentioned by the author in his subsequent writings.—Ed.

(B.) Upon these, unconformably, lies a remnant of the "Black Shale" formation of Tennessee. This is occasionally entirely absent, locally, but its place is indicated on the more southern and south-western margin of the Ozark uplift, as seen in the more western of the northern tier of counties of Arkansas, and probably in McDonald county, Missouri. This terrane thickens north-eastward across Tennessee and Kentucky, toward Ohio, but it is apparently replaced by other deposits on passing northward to Iowa. It is generally considered to be of Devonian age. In its south-western extension, in Arkansas and Missouri, it appears to be earlier than some of the Chouteau rocks, but it is not paleontologically evident that it is equivalent in age to the Devonian rocks of New York.

(c.) Unconformably upon the "Black Shale" formation rests some part of the Lower Carboniferous series, the lowest member of which is the "Chouteau group" of Broadhead, consisting of the three members defined in Swallow's Report of the Geology of Missouri as —

{ "Chouteau Limestone,
 { Vermicular Shales and Sandstones,
 { Lithographic Limestone."

Paleontologically this is a distinct formation, and stratigraphically it is the lowest member of the Lower Carboniferous series of the Mississippi valley. In Illinois and Indiana this is the "Kinderhook group" of Meek and Worthen. East of the Cincinnati uplift it is the "Waverly group" of the Ohio geologists, and in Michigan it is represented by Winchell's "Marshall group."

Faunally these several groups are not strictly identical, but they are tied together by common species, and are probably chronologically of the same age. They appear to be related to each other in the order named, the Waverly and Marshall faunas nearer shore, the Kinderhook a zone farther outward, and the Chouteau a mid-ocean fauna, gathered about the Silurian island now represented by the Ozark uplift.

Above the Chouteau group comes a series of limestones, cherty limestones and cherts with, in some places, shales and sandstones. The main part of this terrane represents the "Siliceous group" of Safford. As represented on the western margin of the Ozark uplift it is distinguishable into two formations.

(D.) The lower of these contains a fauna which apparently includes the Burlington and Keokuk faunas of Hall's Iowa report.

(E.) Above this (and the evidence is not positive but probable pointing to unconformity), comes a higher series of cherty limestones with a fauna described as "Warsaw," as "St. Louis," or under some other name, but as here represented not divisible into distinct groups. This is terminated, or, when absent, is represented by a sandstone which is equivalent in some places to the "Ferruginous Sandstone" of the earlier Missouri reports.

(F.) Above these lie the Coal Measures, which may be subdivided, but for that purpose sufficient data are not at present accumulated.

These six terranes (A. B. C. D. E. F.) are faunally distinct from each other, and in the present stage of my investigations, I am not ready to recognize for those of the upper Palæozoic series in western Missouri, any finer subdivisions of the faunas.

The lines capable of sharpest delimitation are those between the Silurian terrane A. and the overlying terrane which may be either B. C. or D., between C. and D., and between E. and F.

The following is a list of localities from which fossils have been collected by Messrs. W. P. Jenney, of the U. S. Geological Survey, James D. Robertson, of the State Survey, during the progress of the investigations in the lead and zinc regions, and Gilbert van Ingen, under my direction, in the course of investigations for the elaboration of the Devonian and Carboniferous faunas of the United States.

The materials have been submitted to preliminary examination, and the letters in the columns on the right indicate the faunas of the horizons referred to above, to which the fossils from each locality belong, *i. e.*, A is Silurian; B is Black Shale; C is Chouteau group, of Broadhead; D is Middle Mississippian, equivalent to "Burlington" and "Keokuk;" E is Upper Mississippian, equivalent to part of the "Warsaw," and the "St. Louis;" and F is Coal Measures.

LIST OF LOCALITIES AND THEIR FAUNAS

1301.....	Joplin, Jasper Co.....				D	E	
1302.....	Grand Falls, Shoal Creek, Newton Co..				D	E	
1303.....	Webb City, Jasper Co.....					E	
1304.....	Carterville, Jasper Co.....				D	E	
1305.....	Carthage, Jasper Co.....				D		
1306.....	Granby City, Newton Co.....				?		
1307.....	Neosho, Newton Co.....				D		
1308.....	Powell, McDonald Co.....					E	
1309.....	Splitlog, McDonald Co.....					E	F
1310.....	Seneca, Newton Co.....				D	E	
1311.....	Aurora, Lawrence Co.....				D		
1312.....	Ash Grove, Green Co.....			C	D	E	
1313.....	Springfield, Greene Co.....			C	D		
1314.....	Chadwick, Christian Co.....				D		
1315.....	Bolivar, Polk Co.....			?			
1316.....	Fair Play, Polk County.....			?	?		
1317.....	Graydon Springs, Polk Co.....				D		
1319.....	Sedalia, Pettis Co.....			C			
1320.....	St. Louis.....					E	
1321.....	Spurgeon, Newton Co.....				D		
1323.....	Sec. 15, T. 28, R. 33, Jasper Co.....					E	
1324.....	Oronoga, Jasper Co.....					E	
1325.....	Jackson, Newton Co.....				D	?	
1251.....	Sedalia, Pettis Co.....			C	D		
1251D..	Georgetown, Pettis Co.....			C			
1252.....	Clifton, Pettis Co.....			C	D		
1253.....	Sweet Springs, Saline Co.....			C	D		
1254.....	Higginsville, Lafayette Co.....						F
1255.....	Higginsville, Lafayette Co.....						F
1256.....	Higginsville, Lafayette Co.....						F
1257.....	Higginsville, Lafayette Co.....						F
1258.....	Odessa, Lafayette Co.....						F
1259.....	Osceola, St. Clair Co.....			C	D		
1260.....	Odessa, Lafayette Co.....						F
1261.....	Monegans, St. Clair Co.....						
1263.....	Clinton, Henry Co.....						F
1264.....	Clinton, Henry Co.....						F
1265.....	Dover, Lafayette Co.....						F
1266.....	Lexington, Lafayette Co.....						F
1267.....	Osceola, Clinton Co.....						F
1268.....	Clinton, Henry Co.....						F
1269.....	Clinton, Henry Co.....						F
1270.....	Clinton, Henry Co.....						F
1271.....							
1272.....							
1273.....	Northview, Webster Co.....	A		C	D		

The Carboniferous formations are principally represented in the material thus far accumulated.

The Carboniferous system was first formulated by Conybeare in 1822.²

If other names were to take the place of the established but

² Outlines of Geology of England and Wales, p. 320, 1822.

inappropriate designation "Carboniferous system," *Pennine system* is appropriate, because the Carboniferous system of the Pennine range of England was Conybeare's typical locality, as I have previously suggested.³

This Carboniferous (or Pennine) system, as represented in Missouri, may be classified as follows:

"Permo-Carboniferous" = ? Permian series.
 Coal Measures, or Pennsylvania series.
 Lower Carboniferous, or Mississippian series.

The facts before us are sufficient to suggest a threefold division of the Mississippian series on paleontologic grounds, thus:

Mississippian series.....	{	Ste. Genevieve group.....	{	Chester.
			{	St. Louis.
				Warsaw (in part).
	{	Ozark group.....	{	Keokuk.
			{	Burlington.
	{	Chouteau group.....		Chouteau.
				Vermicular.
				Lithographic.

The name "Mississippian series" is a revival and slight adaptation of the name proposed by Professor Alexander Winchell in 1869. He said: "I propose the use of this term (Mississippi Limestone series, or Mississippi group) as a geographical designation for the Carboniferous Limestones of the United States, which are so largely developed in the valley of the Mississippi River."⁴ Up to the date of the publication of Professor Winchell's paper, the Chouteau group of Missouri was still called "Chemung" by many paleontologists, and the great value of the paper consisted in showing that the Marshall group of Michigan, the Waverly series, in part of Ohio, and the formations between the Black Shales and base of the Burlington in the Mississippi valley (which had already been so interpreted by Meek and Worthen), were properly associated with the Carboniferous system on paleontologic grounds. I would propose, therefore, to apply the name "Mississippian series," not only to the limestones, but to all these formations which, in the Mississippi valley, are naturally associated by their faunas with the limestone series, and thus use it as a name

³ Geol. Soc of America, Vol. II: "What is the Carboniferous System" read at the Indianapolis meeting August, 1890. Abstract, Bull. Geol. Soc. America, Vol II, pp. 16-20.

⁴ "The Marshall Group, etc." Proc. American Philos. Soc., Vol. XI, p. 79.

for that particular expression of the Lower Carboniferous series, which is characteristic of the rocks of the Mississippi valley.

The "Chouteau group" of Broadhead is already well defined and consists of the

Chouteau Limestone,
Vermicular Shales and Sandstones,
Lithographic Limestone,

of the Missouri reports of Swallow, Shumard and Broadhead.

The formation c. of my table refers to this Chouteau group of Broadhead, called "Chemung group" in the reports of the first survey of Missouri by Swallow and Shumard; so called also by Mr. Hambach in the list above referred to.

The *Ozark group* (D. of my table) is a group proposed to include the formations heretofore described as Encrinital Limestone, Burlington Limestone, Keokuk group, and their equivalents in Missouri, Illinois, and Iowa, and part if not all of the Siliceous group of Tennessee, all the faunas of which indicate a close paleontologic relationship. It is possible that some of the formations heretofore referred to as the Warsaw group may more properly belong to this group.

The name Ozark group is suggested by the fact of the prominent development of the formations constituting the group on the southern and western margins of the Ozark uplift.

The *Ste. Genevieve group* (E.) is a name and classification proposed to include the formations whose faunas have been described under the names Warsaw (in part), St. Louis, Chester, Kaskaskia, upper Archimedes, Ferruginous Sandstone, and their equivalents in Missouri, Illinois and Iowa; and particularly defined under the name "Archimedes group" by B. F. Shumard in his report on the Geology of Ste. Genevieve County, Missouri.⁵

In his classification this group includes "Archimedes Limestone or Kaskaskia Limestone, 200 feet; Sandstone, 80 feet; Archimedes Limestone, 50 feet; St. Louis Limestone, 150 feet; Oolitic Limestone, 20 feet; Archimedes Limestone or Warsaw Limestone, 80-100 feet." It is underlain by the "Encrinital Limestone" and overlain by the Coal Measures. This group is particularly well developed on the eastern and north-eastern margin of the Ozark uplift.

⁵ Geological Survey of State of Missouri, 1855-1871, pp. 292-293, 1873.



EMERSON LOVING-CUP

EMERSON GEOLOGICAL LOVING CUP

BY CHARLES KEYES

One of the most felicitous events in the history of the Geological Society of America took place at the recent Amherst meeting. The occasion was the celebration of the 78th birthday of Professor Benjamin Kendall Emerson, and the presentation by the members of the Society, of a large and beautiful silver loving cup as token of their deep appreciation for his long and valuable services to American geology. It was meet that the presentation should be made at Amherst. There could hardly be more fitting time and place to extend to him the Society's affections, appreciations and congratulations.

Amherst is Professor Emerson's home town. For nearly half a century he serves well and faithfully Amherst College and its near neighbor Smith College. He is a teacher of exceptional ability, of inspiring presence, and of cultured personality. One of the most active geologists of his times, his laboratory and work-shop is the all out-doors. He is one of the original fellows and founders of the Geological Society, serving as its President in 1899-1900. Few are the meetings of the Society, since its organization 35 years ago, that were not graced by his commanding and picturesque presence.

Presentation of the loving cup occurred at the annual dinner of the Society on Thursday evening, December 27, 1921. The occasion was gracefully presided over by Prof. James F. Kemp, of Columbia University, one of Professor Emerson's old students, and President of the Society. The presentation speech was appropriately given by Dr. John M. Clarke, another distinguished Emersonian student of 1877, and now State Geologist of New York State, and Director of the New York State Museum. In joyous mood Dr. Clarke spoke substantially as follows:

"The worker in Science, if his hand has touched the altar, is a creator

of miracles. With every mystery of nature that opens to his sight there comes into view a longer vista of the still unknown. The paths of science are without end, though not without their reward in truth and virtue by the way.

"The teacher of Science is the personification of immortality, of the continuity of the intellectual germplasm. We honor tonight a great teacher of geological science and there sit about this table men who drew their love for this science and their exhilarant impulse to their life work from him; men who stand on different rungs in the ladder of the years, but who have spread the versions of truth as they learned it here, to unknown hundreds of students who, themselves turned teachers, have sent these echoes flying along in ever widening rings of time.

"The story of Science is the endless story; and another industrious ant brings another grain of fact, and another ant brings another grain, until the facts have piled up mountain high and any single branch of orderly knowledge calls for a gargantuan digestion. There were fewer facts set before us in the days of the Emersonian period. The array of the elementary data of the science was not so appalling as now and, served with all the spice of reminiscent incident and irrelevant story, it became so inviting a repast that we are still feasting at that table.

"The beauty and effectiveness of Professor Emerson's teaching lay in this: His hand had actually grasped the horns of the altar of American geology. He had drawn his inspiration from a learned expositor and intrepid pioneer in the science—a veritable Baptist crying in the wilderness, 'Prepare ye the way of the Lord'—the Reverend Doctor Edward Hitchcock, President of Amherst College and Professor of Natural Theology and Geology. Of Edward Hitchcock's achievements in his science the evidences are here and all about us. Over the entire domain of Massachusetts, throughout the whole length of this historic Connecticut Valley, he opened up the treasure house of knowledge, awakening an eager response and earnest craving for such knowledge from States and Colleges throughout the land. Nearly a century ago the atmosphere of Amherst was impregnated into the fragrance of the budding science and eighty-five years ago Hitchcock was appointed the first State Geologist of New York and entered upon his field, though soon to give it up because it was too far away from old Amherst—reason enough! He stayed here to strengthen that atmosphere, to inspire students with his extraordinary fidelity, and with perfectly open mind to bring the facts of his observation into the harmony of his philosophy.

"Out of this atmosphere came Emerson to his students, his draft of Hitchcockian teachings somewhat bespangled by new lights brought back to this safe nesting place from sojourn and commerce with the savors of Germany; piquant, fresh, beckoning to those who would go further with him. Through him, I think, came inspirations to those who did follow that were really, if unconsciously, echoes of Hitchcock's heroic

days and influence, and those who drank deep at this spring drank pure and strong.

"Tonight we are but moons revolving about our luminary, some owing a direct and filial descent; centrifugal once, centripetal now; and others, many others here, bound by the same tie even though they be less conscious of it. Looking tonight as he did a generation ago, and as Moses looked when he struck the rock for its living water, he recalls the days not only of the lecture-room but those of incessant and arduous work in the complicated fields of Old Hampshire County, which are his by a peculiar right and by an emphasis of interest. Always venerable, never old, a reflector of bright and pungent occasions in this Society when the harsh business of the day left more room for good fellowship; intrepid and honored, he has all our deference and challenges our best emotions. Vergil of the Georgics invoked the "Gods of the Rocks and Soil, my father's Gods and mine!" Of these we here are the messengers to men. Perhaps more than we know, we are so because of Emerson's inspired hammer.

"Professor Emerson, my words to you are merely the sound of the many voices about these tables speaking through my lips. It is the first time that Amherst has shown the full harvest of the seed sown by Hitchcock, and then by you, and no such occasion could be let pass without our erecting here a milestone that should be a memorial of the event and an affectionate recognition of your great service to the science of Geology. Take, then, this gift, with the assurance of its full meaning in friendly remembrance, a symbol of the measure of our regard."

Professor Emerson responded in his happiest vein, yet his voice betrayed deepest emotions. Several of his former students and some of his life-long confrères also spoke feelingly and awakened many tender memories. Among former students trained by Professor Emerson who afterwards attained marked distinction in geological circles are recalled at this moment: Prof. Rufus M. Bagg, of Lawrence College; Prof. John M. Boutwell, of Salt Lake City; Mr. Arthur B. Call, of Pasadena; Mr. James G. Carlton, of Marblehead; Dr. John M. Clarke, of the New York State Museum; Dr. C. Whitman Cross, of the U. S. Geological Survey; Prof. Julius W. Eggleston, of Cuttingsville; Edward H. Emerson, M. E., of New York City; Dr. Henry S. Gane, of Santa Barbara; Prof. Adam C. Gill, of Cornell University; Mr. H. Norton Johnson, of Los Angeles; Prof. James F. Kemp, of Columbia University; Prof. Fred B. Loomis, of Amherst College; Prof. George R. Mansfield, of Washington; Prof. Horace B. Patton, of the Colorado School of Mines; Professor Frederick

B. Peck, of Lafayette College; Prof George B. Shattuck, of Vassar College; Matthew van Siclen, M. E., of the Bureau of Mines.

Two other of the old Emersonian students were conspicuous by their absence. They beckoned from the other side. Suddenly cut down in the very plentitude of their great powers Prof. George H. Williams, and Prof. William B. Clark, through their marvelous observational faculties, their superior enthusiasm for their chosen science, and their untiring activities at Johns Hopkins University, of Baltimore, produced in their only too brief careers 25 per. cent of the leading geological minds of the country, according to Professor Cattell's recently published statistics. This tremendous impulse to American geological mentality may have been after all somewhat hereditary, and perhaps may be traced back to old Amherst. Quien sabe?

Professor Emerson is not a teacher alone; nor a closet naturalist by any means. Amidst full schedules of college duties he is an ardent out-doors lover. All out-doors is his field of endeavor. As a field geologist his work is extensive and important. For many years connected with the Federal Geological Survey, his comprehensive reports on Old Hampshire County, the Holyoke Quadrangle, and the Geology of Massachusetts and Rhode Island amply attest his excessive activities and enthusiasm. Besides these sumptuous volumes he constantly publishes for nearly half a century, until a bare list of titles numbers nearly a hundred. The wide scope of his energies is perhaps no better indicated than by perusal of the subjects of his contributions to geological science.

So, fellow geologists, Ye Editor lifts his glass to the happiness, and the strength and the grandeur of the noble Patriarch of Amherst, beloved Dean of our profession; may he long live and prosper.

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PROFESSOR EMERSON TELLS A GOOD STORY; PLACE, CAMPUS OF TORONTO UNIVERSITY; TIME, AUGUST 11, 1913; HOBBS GETS READY TO LAUGH

EDITORIAL

RETURN OF AN AMERICAN GEOLOGIST

After a rather protracted vacation it is with renewed strength and revived energy for strenuous effort that an American Geologist returns to his field of duties, which for a period of twenty years he so faithfully ministered seemingly to the full satisfaction of his confrères throughout the world. In times like these when universally the blighting effects of after-war conditions are so sorely trying, and scientific endeavor is so severely strained, curtailed, and often actually suspended, encouragement, however small, of intellectual reconstruction in counteraction appears especially opportune.

Publication is made possible at this time mainly through enlightened sentiment emanating from certain college influences in Iowa. It is an example worthy of wider emulation and further adaptation. By a singular, and it may be said almost accidental, combination of circumstances a small but congenial coterie of university men met on an evening not so very long ago in one of the Des Moines clubs. As they gathered before the fire-place conversation chanced to turn upon the topic of how best one might be of some small practical service in the reconstruction of the global intellectual disturbance occasioned by the World War. Financial aid in the dissemination of knowledge appeared urgent, and the satisfaction of this desideratum seemed to occupy a neglected field. First entrance into journalistic demesnes was brought about in happy manner.

By curious coincidence there were present in this little fire-side company old students of the Winchells, Shaler, Le Conte, Williams and Calvin. Blows from a geologist's hammer soon struck sparks. It was proposed, as a sort of memorial to the founders of the *American Geologist* that this journal be revived. Subsequently loyal alumni of several universities congregated and five

of the groups subscribed support for a period of years. Plans were matured for permanent and ample endowment. Editors and managers were drafted from the five representative institutions. The selection of the editor-in-chief was made mainly because of his one-time connection with two of the universities, and his close and long association with the founders of the old magazine. Under auspicious environment and conditions the new magazine comes back not alone to a former field of endeavor and usefulness but to a wider one. Hope is that the trust imposed will amply fulfill expectations.

One untoward incident arises to ruffle the otherwise perfect placidity of the geological waters attending the new enterprise at its launching. Attention to it would not be called to the matter were it not for the fact that generous college men, irrespective of their Alma Maters, are placed thereby in the false position of disloyalty to their home institutions. Iowa State University, to which the great Calvin devoted the best years of his life, will not be represented. As originally planned, the Iowa University was to have editorial representation on the same basis as all the others. In view of the circumstances that it was to be a memorial to Professor Calvin, his life-associate, the then President Macbride, desired that the entire University should share in the plan, and with this suggestion there was hearty concurrence. When it came to effecting the final arrangements, President Macbride having in the meanwhile been made President Emeritus, objection to the making of the magazine a Calvin memorial was made on the part of the University, and also intimation of refusal to take part unless control of the journal was granted. Effacement of the memory of its great men is nothing new for Iowa University. It has happened repeatedly before. The present instance only follows in the wake of the exercise of a similar policy on part of the Iowa Geological Survey. It is a very great pity that petty personal or fancied grievance should be permitted to outweigh the remembrance of a University's grandest personage in all its history. So the seeming disloyalty of college men of the State of Iowa finds full explanation.

In general policy the *Pan-American Geologist* will not be materially different from what its predecessor was in a former period. As in days gone by it serves to continue the role of an open forum.

The fears of a large and long growing body of reactionaries that something new may appear in print will not be calmly countenanced. Interinstitutional jealousies will not be encouraged. Increasing bigotry on the part of those who have text-books to defend, or hold public bureaus holy will be given scant consideration. These are some of the negative influences which are to find active resistance. There will be strong counterirritants to such atrophic tendencies. Ours is not a science *ex cathedra*.

On the other hand, positive encouragement will be ever lent to wholesome speculative discussion, constructive criticism, and creative productivity. The global aspects of geologic record will come in for large attention.

FIRST AFTER-WAR CONGRES GEOLOGIQUE INTERNATIONAL IN BELGIUM

In the reconstruction days following the World War, the greatest contest at arms of all time, it is hardly to be expected that students of earth sciences should be the last to find themselves. They are the first. In this respect they take precedence over the involved governments themselves. Without abandoning their individual patriotism they dull the points of their differences and sink their prejudices in a true brotherhood of man. Were their function only political a League of Nations would stand forth that would begin instantly to carry peace on earth and good will toward men down the avenue of the ages.

That the first after-war Congrès géologique international should be called to meet in little, war-ridden, but lion-hearted Belgium, that one state of all the world, whose safety was guaranteed by the Powers, is most appropriate. Delegates to the new Congress enter upon their duties without selfish interests to maintain, with no international jealousies to guard, and with catholicity of sympathies that nowhere has equal. World questions are alone to be discussed. Problems of less than global scope are to be left mainly to the nations having jurisdiction. The results can only portend good; good that may easily pass beyond the bounds of geology.

The last, or Twelfth, triennial conclave of geologists met in Canada, in 1913. The Brussels gathering next summer comes on time; but two regular sessions slipped by because of the interna-

tional turmoil. Next Congress comes upon the field with feelings of closer union than has been possible heretofore, with ardent desire to deal with large problems, and with convictions that it should broaden its policies. The American member of the organization committee, Doctor Smith, notes the following resolutions for simplifying the activities of the international gathering: (1) That the International Geological Congress should be continued, modified to meet present conditions; (2) that the question of the establishment of an international Geological Union should be considered at the next International Geological Congress, and that it is undesirable that any steps should be taken until the question has been so considered at a full and representative gathering of geologists; (3) that the organization of the Congress shall be as simple as possible consistently with securing continuity of effort from one Congress to another; and (4) that the above resolution be communicated to the President of the Geological Society of London, the Secretary of the Royal Society, and the Secretary of the Belgian organization committee of the next International Geological Congress.

The hundred odd geologists from America who attend the Belgian Conference next summer will be especially interested in going over the geological setting of the Great War in France. Never before was battle fought where geological features were so obviously such decisive factors. Never before was it so compellingly demonstrated that offensive battle should be fought with the "grain" of the country instead of across it.

The myriad parallel cuesta ridges of the Somme and Marne battlefields form ramparts which no human ingenuity can match. Had Hun hordes but followed the young Crown Prince's lead and marched on parallel lines with the country's ribs upon Paris the whole future of Europe might have undergone magical change. The entire globe might have been today Teutonic, a German Kaiser might have been ruling on the banks of the Potomac, and a holy monarchy might have been reared over the bones of him who a century before left behind the purest name in all history.

But German High Command made defenseless, neutral Belgium outweigh all other considerations. It chose the easy beginning; but in nearing the French Capital Teuton armies struck transversely the cuesta barriers against which mightiest barrage proved as in-

effectual as bombardment of marbles, and against which successive waves of German bayonets broke in perfect impotency as the storm sea dashes itself to spray under the beetling cliffs of Albion.

The great lesson must be read directly from the book of Nature, on the very ground itself, with the matchless panorama of cuesta France spread out before.

GEOLOGICAL MENTALITY IN THE MAKING

For the ten year period covering the second decade of our new century, the death toll of distinguished scientific men is so very appalling that sometimes our gravest apprehensions are aroused as to whether there can ever be replacement adequate enough to overcome their complete extinction. We find partial response to our fears in Professor Cattell's recent, new and third edition of "American Men of Science." Redistribution of the One Thousand leading scientific minds, determined by vote of the recognized authorities in each of the twelve principal branches of science, brings out a number of startling yet very instructive features.

Astounding fact is it that almost one-half of the One Thousand "starred" men are intimately identified with so few as two institutions; and that 800 of the 1000 emanate from only six universities. Recognition of starred men who are not products of cloistered hall is a distinct advance, and is one of the direct broadening results of the World War. Amounting now to over five per cent of the total, the number who are not in any way associated with institutions of learning probably reaches double the figure mentioned, and will doubtless increase with the passing of the years. This leaves only about 15 per cent of the "stars" of the country to be apportioned among the 400 odd other colleges. The fundamental significance of this circumstance is impressively obvious.

That 80 per cent of the creative productivity of the nation should be concentrated in half a dozen universities is a fact that is itself most remarkable. It is an increase of 50 per cent in the decade.

Compared with the older table of ten years ago the new alignment is as follows: there being, of the One Thousand leading men of science, produced by

	1910	1920	Increase %
Yale University.....	37	74	100
Cornell University.....	44	79	80
Columbia University.....	63	100	60
Chicago University.....	80	113	40
Harvard University.....	79	190	140
Johns Hopkins University.....	245	243	0
Totals.....	548	800	46

The unprecedented gains of the six universities at the expense of the rest of the country merits closest attention. Even more phenomenal is the unabated prestige of the Johns Hopkins. That this university should hold its own in the face of most discouraging internal conditions which might be easily avoided is one of the genuine marvels in the annals of American higher education.

Than these figures of the six universities there could be no more complete answer to Thomas A. Edison's recent foolish, albeit widely accepted, tests of college education, as well as reply to the North American Review authors of "What do College Students Know?" and "What do Teachers Know?", and to all others who sneer at the Ph.D., and regard the gormandizing acquirement of unrelated facts rather than sound and clear thinking as the sole end of college training.

Percentage losses of the State universities are tremendous, but their force is somewhat mitigated by the fact that the actual numbers involved are not large. In the mad race of these institutions for supremacy of numbers of college students the post-graduate body necessarily suffers. For instance, one university of this class blatantly boasting of over 500 students in its geological classes, produces not a single star in a generation. It is like the famous pontoon bridge over the Golden Horn where a hundred thousand people cross each day, but not a single thought in twenty years. What else is to be expected of a department when the head himself is not a producer, much less a "star." If they expect to sustain themselves along the lines of copious and creative productivity it behooves every one of these state universities to strengthen its teaching personnel by importation of at least one instructor of recognized supremacy for each of its twelve departments of science.

The strong retrograde movement of the State universities in the matter of national mentality, as indicated by the new statistics,

is no doubt largely ascribable to the peculiar makeup of irresponsible governing boards. Their members are not usually drawn from university circles, are commonly unsympathetic, and wholly untutored in even the fundamentals of higher educational training; their only claims to mention in "Who's Who" rest on some fancied executive quality entirely foreign to their natures; they thrill at hullabaloo rather than the Ph.D., and consider only the Brawn Goddess of college worship.

But the State Universities are not alone in this respect. At least four of the "Big Six" are likewise afflicted; and probably the majority of the 400 other colleges and universities. In the instance of the famous Baltimore institution the fact that a quarter of a century's determined effort to replace a truly national university by an indifferent city trade-school brings not the anticipated results is high tribute to the tremendous sustaining power of the Gilmanian afflatus. It is, as in the Roman state of old, which after the passing of the noble Roman himself, continued to function for three centuries in the hands of bondmen, slaves and foreign peasants.

Analysis of the "new blood" transfused to the One Thousand leading men of science reveals the curious fact that measured by the "stars" contributed, first strength in eight departments rests with the Johns Hopkins university, and the remaining four of the twelve departments go to Harvard. Guaged by the same criterion Yale's strong department is Geology wherein it ranks with Chicago and Johns Hopkins; yet half of her departments contribute not a single unit. Columbia's leading departments appear to be Zoology, Geology and Psychology. Cornell's are Botany and Geology. Chicago has five strong departments with that of Geology ranking as her best. Among the "Big Six" institutions more than one-half of the departments contribute either none or no more than a single man.

Still another point of surpassing interest is the geographic origin of American scientific men. It is a strong contrast of city and country. Somewhere President Elliot observes: "The country breeding gives vigor and an endurance which in the long-run outweigh all city advantages and enable the well endowed country boys to outstrip their city competitors." Effete Baltimore, with her great university within her narrow bounds, furnishes, aside

from medicine, not half a hundred scientists and a scant half dozen starred men; while clodhopper Iowa, half-way across the continent from the "Big Six" universities, supplies nearly 400 men of science, no less than 35 of whom shine in the One Thousand class.

Perhaps we get clue to this great riddle in a chance remark of that eminent English naturalist, Alfred Wallace, when he writes: "As I found that amid the distractions and excitement of London, its scientific meetings, dinner parties, and sight-seeing, I could not settle down to work at the more scientific chapters of my 'Malay Archipelago' I let my house in London for a year, from midsummer 1867, and went to live with my wife's family at Hurstpenpont. There, in perfect quiet, and with beautiful fields and downs around me, I was able to work steadily. * * *"

MY LAST MEETING WITH GILBERT

A few months before his demise I quite unexpectedly came upon Grove Karl Gilbert one evening in Berkeley, California. Accidental as was the meeting it was one of the memorable events of my experience. Lawson and I entered the Faculty Club, at the State University, for a little visit together and an old time chat. At this very moment, it seemed, the veteran geologist happened to be at play at billiards, a recreation which on an occasional evening appeared to have caught his fancy in later years. Evidently spying us first through the archway he dropped his cue and immediately greeted us in the lounge. Expressing surprise at finding him there he explained that he was making this secluded spot his residence while he was at work on his Sierra monographs. That evening he seemed to be in an unusually joyous frame of mind. More jovial and entertaining he certainly never was. Conversation sped on in ever widening circles until dinner was served; and then there was also feast that was not corporeal.

That evening our nestor of American Geology appeared at his best. Other days came back. Octogenerian turned youth again once for a night. At dinner table he grew resplendently reminiscent. The larger lineaments of earth drew near. Geological details took on global aspect. Despite the fact that tripartite debate became at times spirited and even acrimonious it left no

sting. The repast ended, the plates removed, cigars lighted, but conversation flowed on and nowhere lagged for even a moment. At length the air thickened, the lights grew fewer, the glow faded, yet still the little company seemed reluctant to move. Greek and Roman, Chemist and Engineer, Literateur and Artist sat on — mute, admiring, intently listening. Bells of the nearby campus campanelle chimed the hour of midnight before the charmed circle was broken. So entertaining were the Arabian Nights tales as told by gracious scientist. Not once did slightest hint of *Weltschmerz* escape the lips of aged master.

Dwelling upon phases of the World War he related with infinite zest the ludicrous results of the international jealousies displayed in this country during the Fifth International Geological Congress, and how Teuton and Frank came well-nigh wrecking all of Major Powell's well-matured plans just before the excursion reached the Grand Canyon. Thus early had the seeds of greatest social disturbance the world has ever known begun to germinate even in the farthest corner of the earth.

Notwithstanding the fact that broader earth problems, as Gilbert expanded upon them, were so fascinating, the younger generation could not refrain from drawing out of him all they could on the Great Basin region. By skillful questioning and suggestion it kept him close to his early exploratory experiences with the government surveys, the topics on which we most desired first-hand information from the savant who had first done so much there. We wanted expression of matured reflections after a quarter of a century of severest test and after introduction to entirely new epicene forces. The long military treks of the Wheeler corps, Lake Bonneville, the Henry laccoliths, the Basin Range structures, Grand Canyon erosion, isostatic intensity in the Great Basin, all passed in realistic review, and received graphic portrayal by master hand. How widely different were musty tome and sparkling speech.

It was with considerable trepidation that we broached the subject of climatic conditions in late geological times in connection with Lake Bonneville. Very much to our surprise Gilbert frankly admitted the inadequacy of the glacial hypothesis because of the demonstrated preglacial existence of the lake, and intimated a conclusion forced in the excitement of proving the duality of

the Ice Age. Inclination was now towards the incidental suggestion of Professor Davis early made that an orogenic explanation should be carefully examined.

In the explanation of the old lake's genesis orogeny which abounded in utmost profusion all about had no part. It was too evident that it was at this late date hard to realize that the great body of inland waters of which the Great Salt Lake of today was an all but vanquished vestige might not have been after all an anomaly among desert features; or that it might represent merely a special phase of a through-flowing stream which was not quite large enough to negotiate the orogenic barrier which chanced to arise athwart its path, while its companion and parallel stream, the Green River, re-enforced by the Grand and other eastern tributaries, was sufficiently powerful to hold its own against all vicissitudes and to carve out through the rapidly bulging Colorado Dome a Titan among chasms. Blocked by such a formidable rampart, the old antiquated river spread out far and wide over the adjoining intermontane plains, until, finally, robbed of its headwaters through diversion by lava flows, it could no longer furnish the lake with its chief supplies, when disiccation set in with marvelous rapidity.

When I remarked that these factors did not matter much and that the great service of his Lake monograph still remained unimpaired he was visibly touched and naïvely expressed deep appreciation that we should think so.

On the Basin Range structures Gilbert's thought seemed constantly to dwell; and he returned to the topic again and again. The theme appeared to greatly trouble him. He expressed desire to review his early hypotheses from the foundations up. He manifestly was in the very midst of it then. When on the galloping Wheeler military jaunts which were so little adapted to close geological observation, the theory originally flashed across his mind, as a meteor flashes athwart the sky. And like the meteor's basic message on its composition and its orbit the fault-block hypothesis was an impression, and its fundamental elements always remained undetermined.

The conception that the Basin Ranges were tilted fault-blocks, floated on the liquid interior of the globe, and tumbled about much like ice-cakes in a river at the time of the Spring break-up,

was, indeed, a brilliant one. It fitted to a nicety the Powellian policy of geological *saisissement*. In after years, in the heyday of the Powell régime in Washington I remember well how proud the old Major was of the generalization that crustal shortening was mainly accomplished by fracturing rather than through flexing as had always been generally regarded. Supreme complacency with which he always pointed to the fault-block theory served only to obscure the fatal incongruity that the principle should be restricted to so inconsequential a portion of the earth's surface. Like Goethe's dazzling portrayal of the origin of the skull from distorted proximal vertebræ it was a poet's flight of the fancy.

Past criticism on some of his brilliant concepts evidently disconcerted him not a little, and even shook somewhat his faith in the correctness of his early conclusions. When he wrote his sumptuous monographs on Great Basin topics there was no such thing as a special desert geology, and geographic cycle in land sculpture was not yet born, and deflation under the stimulus of aridity was not yet a recognized erosional activity. These were things which seemed to disturb his peace of mind; and he plainly showed that he had not explained all. It was evident that in the light of more recent advances in geological knowledge that some of his results would have to be recast somewhat. There was full realization of quiet passage of early radical to the late conservative, even as Jerome in painting had brought home to him at the end of only twenty years.

Two features in particular appeared to me remarkable. These were his last word on laccoliths and in isostasy. Mention of the possible play of tectonic stress in the formation of laccolithic intrusions, as has so recently been proposed for the Sierra del Oro of New Mexico, illicit the astonishing admission that this had lately occurred to him in regard to the Henry Mountains. This thought had awakened a keen desire to revisit that group and look for evidence along lines parallel to the great Water Pocket flexure. When the next morning he took breakfast with me at the hotel he had already formulated a plan whereby Lawson, Willis, Branner, and myself, and perhaps two or three others, might go with him at first convenient opportunity to southeastern Utah and test a new working hypothesis in the field. Too far advanced then was the season for such a trip and the next year he removed to Michigan never to return.

Especially impressive was the discussion on isostatic compensation, as Dutton, Powell, and himself had conceived it. On him was already beginning to dawn the fact that the fatal objection to the Survey's notion of Basin Range structure was a perfect independence of relief and tectonics. This factor had been overlooked in the early considerations. It now menaced the entire explanation. Days afterwards this discussion revived in me and awakened the idea that the main difficulty had been that the geological datum of isostasy was itself mobile.

That last meeting, as it proved to be, with Gilbert was indelibly impressed upon memory. Well weighted down with his four score years his movements were still alert, his eyes were still piercing, and his conversation still scintillated as of yore—a noble form spared to us as a reminder that there were real giants in those days.

Recurrence of that happy meeting was not to be. Winter blasts brought word of his gentle passing. Although of another generation his magnetic fervor of the reformer, the polished fluency of his speech, and the trenchant aptness of illustration betokened the well rounded man, with broad culture, catholicity of sympathies, and generous instincts. Even at his age a few months later one might truly exclaim in the words of the venerable Cato: *Itaque adolescentes mihi mori sic videntur, ut cum aquæ multitudine flammæ vis apprimitur.*

PALEONTOLOGICAL GEOLOGY

Derivation of South American Faunas. The prime controlling influence on the development of the mammals is food supply. Although mammals originate as early as Early Cretacic time, their developement in variety and numbers begins in Early Tertic time, and parallels the development of hardwood trees, and insect pollinated herbaceous plants. Previous to the Eocene deposition, South America and North America were connected. About the time that the Wasatch beds were laid down, South America was isolated and so long remained. On this island continent the mammals, at least, developed independently and adapted themselves to all the varied environments there, uninfluenced by the progress being made on the northern complex of continents, North America, Asia and Europe. Many times these adaptations are parallel, but in all such cases careful study emphasizes the independence of the southern forms.

So much has been written postulating land-bridges across present ocean basins, and migrations especially from Africa and Australia, that it is fundamental to look closely at the fauna to see if it can be explained without recourse to these tremendous revolutions of the land.

The first South American fauna is that of the Notostylops beds. It includes marsupials, edentates, litopternas, typotheres, toxodonts, etc. Marsupials in the south include true didelphids, sparassodonts, caenolestids, and diprotodonts. The didelphids run back to the Notostylops beds but are known equally early in North America and Europe. Without much doubt they found their way south before the isolation of South America. The sparassodonts include large carnivorous forms similar to the Tasmanian wolf, but their history extends back in South America to the Eocene and they have not been introduced since that time. The caenolestids are tiny forms, often called pseudo-diprotodonts.

They are supposed to have been independently developed from some didelphid, but the line is continuous back to the *Notostylops* genus *Progarzonia*, which is just as clearly diprotodont. Then there are the other diprotodonts, *Palaeotheres*, *Pilchena*, etc. Gidley studying the marsupials of the Union beds of North America shows that *Ptilodus* is a marsupial, and that the *Allotheres* and *Multituberculates* also belong in this group, and although not ancestral are closely related to the diprotodonts. This makes it clear that the divisions of the marsupials were established very early, and there is no real difficulty in deriving the South American forms from early representatives of the group on North America.

The edentates are already in South America at the time of the *Notostylops* beds. Wortman long ago showed that *Psittacotherium*, *Onchodectes*, *Conoryctes*, etc. were edentates from the Puerco and Torreon of North America, so that these seem to come from a common source.

The rodents are perhaps the most puzzling group, for they appear suddenly as full fledged rodents in *Paramys* in the Wasatch of North America, and as suddenly in South America in the Deseado (Oligocene), but in the latter case they are hystricomorphs.

Primates are scarce as fossils, but the Puerco of North America yields *Indrodon*, from which, or from related form, the Oligocene *Clenialites*, and the Miocene *Homunculus* seem to be derived, the latter showing already the characteristics of the *Cebidæ*.

Among the herbivores, North America in the oldest Eocene beds had several genera, grouped to make the *Condylarthra*. In like manner the Eocene beds of South America have a large variety of ungulates which Ameghino placed in this condylarth group correctly. In North America there rose from this primitive group perissodactyls and probably *Amblypoda*. In South America this group gave rise to *typotheres*, *litopternos* and *toxodonts*.

There is then, nothing to involve a land-bridge across the oceans; on the contrary the early forms in both North and South America seem to have been closely related and derived from a common source. Review of the reptiles and the fish, especially the Eocene ones, shows also a very close relationship between the two continents. These leave the arguments to those based on land snails

and insects. Both these groups have practices whereby the animals can go for long times in a dormant condition, the snails withdrawn in their shells, the insects in a pupa condition or even in hibernation. These conditions make it fairly easy for the animals to be transported long distances. Considering the directions of the oceanic currents and the distribution of these groups in South America, it appears patent that this was the manner in which they were distributed. The evidence from the *Glossopteris* flora needs very careful scrutiny before being used. It is not known yet whether the peculiarities of this flora are due to climatic adaption or not, and until more than the shape of leaves is used for comparison it is not a safe line of argument for postulating connections from one continent to another.

LOOMIS

Stratigraphy of Black Hills Tertiaries. The recent discovery of a distinctive Cretacic marine fauna in the midst of a thick rock-section northeast of the Black Hills that was long regarded as a freshwater deposit, Tertiary in age, renews that long standing controversy over the proper stratigraphic horizon on which to separate Cenozoic from Mesozoic sedimentation.

In North Dakota a formation called the Lance in Montana, is divisible into three members. The lower terranes have close biotic affinities with the overlying Tertiary Union beds, but the upper member carries the typical Fox fauna which normally occurs far beneath. The section is:

White River sandstones.....	Feet 140
<i>Unconformity</i>	
Union shales.....	1000
Cannonball (Marine) shales.....	300
Ludlow lignites.....	350
Blackhorse shales*.....	500
<i>Unconformity</i>	
Fox sandstones.....	400
Pierre shales (exposed).....	500

The diastrophic setting is that of marked crustal uprising followed by notable down-sinking, expressed by a retreating sea succeeded by a moderate advance over slightly tilted shore deposits. It is probable that when more careful inspection is made than has

* Blackhorse Butte is a conspicuous landmark in Schanasse County, South Dakota, and overlooks the Grand River Valley, and the basal shales of the Lance.

been done there will be disclosed planes of unconformity between the Ludlow shales and the Cannonball shales and also, farther to the west, between the Ludlow shales and the Union beds, as well as between the Cannonball shales and the Union formation. The relationship of the several terranes are indicated in the annexed diagram.

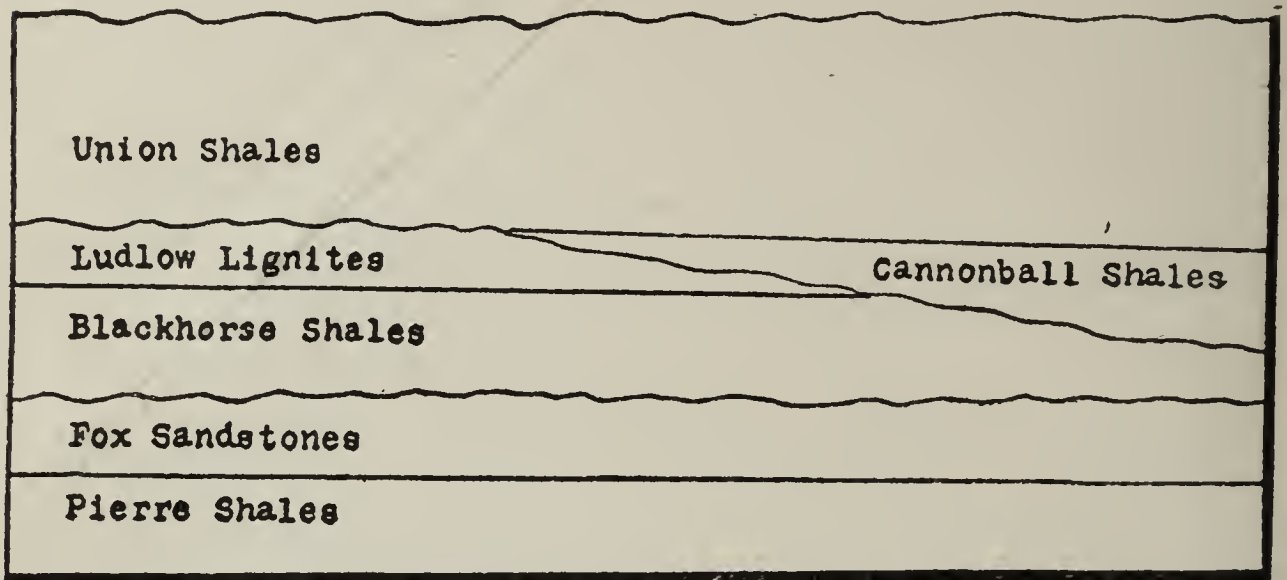


Fig. 5. Stratigraphic Relations of Black Hills Tertiaries.

The vertebrate paleontologists would draw the basal line of the Tertiary at the horizon of the upper unconformity. The invertebrate authorities prefer the same boundaries. Paleobotanists find the floral remains identical throughout the interval between the two unconformities. The stratigraphers, backed by ample and what seems to be decisive diastrophic data, regard the lower unconformity plane as the major break in sedimentation and one of very wide extent. The conflicting lines of evidence make the problem involved one of the most interesting stratigraphic questions on the continent today.

KEYES.

Affinities of the Cannonball Fauna. The organic remains contained in the Cannonball formation with the exception of five brackish-water types, are strictly marine forms and include 2 species of foraminifers, 6 of corals, 60 mollusks, and 2 sharks. Notwithstanding the fact that the molluscan forms have a modern aspect on account of the absence of the ammonoids and other distinctively Mesozoic types they are connected with preceding Late Cretacic faunas of the same region by the specific identity of no less than 40 percent of the species. No strictly Eocene species are

identified except one form from the Union beds. The conclusion is reached that the Cannonball marine beds, and consequently the whole of the Lance formation are Cretacic in age.

Among the 60 molluscan forms 33 are regarded as new; and 27 are identified with previously described species. Eighteen of the species are found in the Fox Hills formation. Twelve of the forms occur in the Pierre shales. To the westward, in the Rocky Mountains, six species are found in the Bear Paw beds of the Pierre; and three species appear in the Laramie brackish-water beds. One form was previously taken in the basal layers of the Union terrane.

It is evident that a large element in the Cannonball fauna is directly descended without specific modification, or with only slight change, from the preceding Cretacic faunas of the Rocky Mountains and Great Plains regions. These Late Cretacic faunas display progressive modernization due to the gradual elimination of distinctive Mesozoic types and the concurrent introduction of modern generic forms which continued through the Tertiary times and are still living.

The Cannonball fauna is really a Fox Hills fauna from which certain characteristic Cretacic types have disappeared, and which was farther modified by the immigration of a number of other types that were not present in the Fox and Pierre seas of the Great Plains region.

STANTON.

Lance and Union Formation are Mesozoic in Age. In the Black Hills region paleogeographical maps bring out the fact that the Cannonball terrane is a product of the final Cretacic seas, since there is no possibility of connecting this marine section with any of the Gulf Eocene, or the Eocene of the Pacific coast. It is therefore the marine equivalent of the Lance formation.

Inasmuch as the Lance and Union sections are continuous formations, have wholly archaic mammalian faunas, and are broken by a period of orogeny and lack of deposition from the succeeding Eocene deposits, with their entirely different and modernized mammals, the line of separation between the Mesozoic and Cenozoic apparently lies between the Union and Wasatch formations and not between the Fox and the Lance beds.

From this conclusion the paleobotanists will, of course, dissent, but we have now come to the parting of the ways. Our floral brethren will continue to say that the Cenozoic begins with the Lance strata, but the dominating faunal evidence of both the invertebrates and the vertebrates, backed as it is by the field relations and the assumption of two movements of the Laramide revolution, binds invertebrate paleontologists and geologists together in the conviction that the Lance and the Union beds were laid down during Mesozoic times. SCHUCHERT.

Tertiary Aspects of Lance Beds. The question of the ages of the Lance and Union sections is a part of a very much larger problem involving a conception of the geologic evolution of the whole Rocky Mountain province. More than a score of minor formations younger than the great wide-spread Cretacic section and older than the Wasatch Eocene beds are to be correlated and interpreted. These formations present much varied evidence concerning the history of the Cretacic-Eocene transition epoch.

The old idea of diastrophism which characterized the transition from Cretacic to Eocene times is faulty. The change was gradual rather than sudden. Although over a large area Cretacic deposition was ended the uplifting is epeirogenic in character and sedimentation is prevailingly continental in nature. Under these circumstances environmental change affecting life is by no means abrupt as is often assumed. In general the newer picture of Rocky Mountain development after Laramian time gives no basis for the belief that dinosaurs and some other Mesozoic land types could not have survived into the Eocene. Indeed, some of them did exist through the long interval when the entire Cretacic section was being removed from over a large part of Colorado and adjacent regions. The Lance formation rests in some places with erosional unconformity upon the Cretacic Fox beds. The gap is of undetermined extent; it may be large instead of small as is sometimes claimed.

The Cannonball shale which separates the non marine Lance beds from the Union section demonstrates temporary return of the sea from an unknown and as yet undetermined region, probably from the north of the Black Hills area after an absence of considerable duration.

What is needed in place of the two paleogeographical maps reproduced in the Stanton report is one, or several of them, to express a reasonable hypothesis of the course of retreat of the sea as the land barrier rose and apparently cut off entirely the restricted northern ocean from the Mexican Gulf.

CROSS.

Floral Continuity in Lance and Union Sections. The problem of establishing the divisional line between the Mesozoic and Cenozoic formations of the Rocky Mountain province is one of the geological storm centers of years. The question can only be settled when all available sources of evidence are evaluated and harmonized. Drawing the line at the top of the Union beds of the Black Hills region profoundly affects other areas and other equally important problems, the true significance of which is either underestimated or completely overlooked.

The flora of the Lance formation is unmistakably a Union group of plants. It ranges through the entire vertical extent of the older terrane. Some of the most characteristic Union species occur within a few feet of the base of the section. Union beds are commonly regarded as Eocene in age. They yield a very large variety of forms, approximately 500 species. The flora of the lance terrane is also a rich one, comprising about 125 species, eighty per cent of which are found also in the Union strata. Of the entire Lance-Union flora 15 species only are reported from the Cretacic rocks.

Sedimentation appears virtually uninterrupted through the Lance and Union successions. From the floral data available it is utterly impossible to draw a line satisfactorily separating the two formations. It is sometimes assumed that there is here a continuous and unbroken sequence of deposits extending from the Pierre and Fox formations to the top of the Union section and that reported erosion planes, which occur between several of the formations are due to nothing more than changes from marine to brackish-water and fresh-water conditions, or to irregularities characteristic of epirotic deposits, the local breaks not representing a loss of geological time of any notable historical value.

The plants certainly do not sustain this contention. They strongly point to a very considerable hiatus between the acknowledged

marine Cretacic section and the fresh water Lance deposition. The Laramian series is not known within this area. Can it be doubted that it is the interval during which in other regions beds of Laramian equivalency were laid down and subsequently removed in whole or in part? That there is an important interval of some kind is also shown by the fact that it was sufficiently long for more than 60 per cent of the marine Cannonball forms to be derived through modification from the typical Fox fauna.

KNOWLTON.

Phyletic Relations of Lance Vertebrates. The Lance vertebrates are strictly Cretacic types. The fauna is clearly a continuation and a specialization of the Judith (Late Cretacic) fauna. It contains no new elements. As shown by the amount of evolutionary change in many phyla it is considerably later in point of time than the fauna of the Judith and Edmonton beds.

The earliest placental mammals are at the Puerco horizon, which may be as old as the Lance, or older. The true Tertiary mammalian fauna appears suddenly at, or near, the bottom of the Wasatch section. It is a distinctly new fauna; and consists mainly of the modern orders. The great faunal break lies at the end of the Paleocene, with the increasing of Cenozoic vertebrates at the base of the Wasatch section.

Of the two leading criteria generally followed in the faunal classification of geological terranes first appearance of new groups seems more logical and practical than extinction of ancient types. By this standard the Wasatch fauna is the introduction of distinctively modern, or Cenozoic life, the preceding faunas, even including the Paleocene placentals being essentially the last vestiges of Mesozoic life.

When the several lines of biotic evidence are so conflicting is it not possible by mutual concession to adopt some compromise? The top of the Union section appears to have best elements of mutual acceptance. It is in conformity with the historic and common European usage. It conforms to the insistence of the paleobotanists that the Lance and Union sections should be kept together. It seems to give a satisfactory base for the stratigrapher in the wide-spread and characteristic Wasatch formations. It places all the dinosaur formations, and the bulk of the "Pale-

ocene" faunas, in the Cretacic where the former certainly belong and where the latter probably should be relegated. It assigns all the late Paleocene faunas to the Tertiary.

The replacement of the Cretacic by the Tertiary vertebrate fauna would thus be a little later, and the substitution of the Late Cretacic by the Tertiary flora a little earlier, than the horizon agreed upon.

MATTHEW.

Physiographic Setting of Earliest Tertiary. The Rocky Mountain province is one which from earliest geological times has been one of the mobile tracts of earth. No similar belt on the face of the globe has so many diastrophic movements so plainly recorded. Abundant evidences of post-Paleozoic oscillations of the earth's crust are still retained in the physiognomy of the Cordilleran uplift.

Although in single rock sections it is rarely possible to evaluate in time units the magnitude or duration of erosion intervals as represented by unconformity lines there is no uncertainty on this score among the plantation levels of the southern Rockies as recently determined. There is no room for difference of opinion regarding whether a given orogenic movement belongs to a major or a minor category. The stratigraphic geologist is left in no confusion as to where to draw his taxonomic lines in the terranal column.

Of the post-Paleozoic revolutions four are certainly in the major class for they are almost continental in extent. Comanchan peneplanation reached from the Mexican tableland to the Canadian shield, and from the Pacific Ocean to the Mississippi River. The one distinguishing the Cretacic Period is hardly less extensive. It is known over the Cordilleran province as the Raton peneplain. Its erosional activities continued through Laramie times. In places the entire Cretacic section is removed. Its all but vanquished vestiges over the Rocky Mountain uplift of today are in the peak tops which rise above the summital plain.

Along the Rocky Mountain front in the Mesa de Maya district especially, the Raton peneplain bisects horizontally a mile thick coal-bearing section that was long regarded as a Laramie succession. But it itself really represents a Laramie hiatus. Above its level is a great thickness of epirotic deposits which are still

beneath the oldest Eocene beds as generally recognized, and which, according to the paleobotanists, carries a distinctive and extensive Tertiary flora. This is a planation plane which seems to extend northeastward beyond the Black Hills; and although there rather inconspicuous has the Lance formation resting upon it. In the Black Hills region, therefore, it is surely a major hiatus, perhaps the greatest that we know on the American continent.

The next great planation level is the Maya plane which is the beveled summit of the lofty Mesa de Maya, preserved by old basalt flows. It is Miocene in date.

Whatever is the separate testimony of the vertebrates, the invertebrates and the plants the great interior continental planation on which is ushered in Tertiary sedimentation in the Black Hills is that recorded by the unconformity lying at the base of the Lance succession.

KEYES.

Basal Tertiary in Rocky Mountain Region. When Arduino of Padua, in the middle of the Eighteenth Century, proposed the geological title Tertiary, it was intended to cover the unconsolidated, highly fossiliferous beds which rested in marked distinction upon, and were in part derived from, the indurated marine strata which were later denominated the Cretacic. Between the terranes of these two periods, in England, a marked stratigraphic break was also early recognized, which by many English scientists was regarded as the greatest hiatus in all geological history. Without much modification of conception this latter view was transferred to America. But a great erosional hiatus implies of necessity concomitant deposition elsewhere.

In the Cordilleran region we find something of the depositional equivalent of the basal unconformity of the Tertiary elsewhere. Along the Rocky Mountain front there occurs a full half-mile of strata which are older than the oldest Eocene beds. This is a section much more imposing than the entire Tertiary succession above the base of the Eocene in most other parts of the continent. In Colorado and New Mexico Shoshone, Denver and Ratonan are some of the terms applied to different parts of this great pre-Eocene Tertiary sequence. There is still a major unconformity at the base which appears to represent even more than the entire Laramie sedimentation to the westward.

With this ponderous pre-Eocene succession of Tertiary sediments delimited the determination of the full depositional equivalent of the basal hiatus displayed elsewhere becomes one of the problems of prime import. For this part of the general rock-column the geological record is unusually complete. This is, of course, what makes the Rocky Mountain Tertiaries so supremely interesting at the present time. It is the diastrophic aspects of the problem which demand first consideration.

When, then, there appears an intercalation of marine beds carrying Cretacic fossils, as represented by the Cannonball formation of the Black Hills region, the time element finds closer and closer adjustment. In the region to the west the entire Cretacic section is in some places removed before the laying down of the pre-Eocene deposits. Marine sediments with remains of Cretacic types of life may therefore, easily exist amongst fresh-water or epirotic deposits that occur above the unconformity plane which marks the great epeirogenic movement which initiated the Tertiary time in the region.

One feature which does much to confuse Cretaco-Tertiary stratigraphy in the Rocky Mountain province is the lithologic similarity and presence of coal beds. All such are commonly referred to the Laramie formation. In the Mesa de Maya district of north-eastern New Mexico, for example, the coal-bearing section called Laramie is 4000 feet thick, yet we now know that not a single foot of it is of that age. The lower half of this section is Montanan, the midway unconformity represents the span of the Laramian, and the upper half is Ratonan or Tertiary. It is a section identical with that of the Lance-Union sequence of the Black Hills, except that there is no marine member present.

KEYES.

Orötaxial Relationships of Lance Series of Montana. In our general scheme of taxonomy of geological formations the sea gives us our continuous record which diastrophic oscillation divides into various groupings of episodes according to the amplitude and expanse of the crustal movements. Thus is made possible a classification of rock terranes that has a strictly physical basis and that is entirely independent of any biotic or mineralogic features which the formations may possess. The advantages

which such a schematic arrangement of terranes may have over any other lies in the circumstance that it is genetic in its nature, and that it has as its foundation the very same agencies which make the recognizable changes in sedimentation possible.

Diastrophism finds best expression in mountain uplift. The preservation of its record finds best expression in peneplanation and in the more ancient erosional unconformity plane. From these unconformity planes new sedimentive records arise. With extensive geological mapping accomplished a continental network of unconformities is produced for the whole geologic column. The taxonomic value of each unconformity is then directly proportional to its lateral extension.

Terranal classification becomes feasible without dependence upon faunal or floral content. Such a classificatory scheme is as much superior to the biotic one as was the latter over the lithologic one a hundred years ago.

Analyzing the relations of the Lance formation according to its diastrophic affinities, recognizing the unconformity at its base as one of major rather than minor order, and taking cognizance of the fact that no other major planation occurs in the continental interior until Miocene time the Lance formation falls easily and naturally into the Tertiary section of the regional geological column.

This may not agree very closely with the course of vertebrate evolution, or invertebrate development in the region, but rock classification based upon either of them is not terranal classification at all, but merely a faunal one. There can be no genetic connection between diastrophism and biotic expansion. At best the latter may only be slightly modified by the former. So, with irreconcilable differences between the several biotic lines of evidence in the Lance-Union controversy, as well as many other similar ones, the time and opportunity seem propitious for the geologist in all taxonomic problems to put chief stress on the diastrophic movements, and break away from the antiquated and unrelated biotic criteria in terranal classification.

From the viewpoint of the diastrophist the inclusion of the Cannonball formation with the Lance succession is clearly a mistake of observation. It is hardly the marine equivalent of the Lance section, but altogether younger. It seems to be rather the

depositional equivalent of the Lance-Union conformity, obscure as that line now appears. The Union, Cannonball, and Lance formations manifestly represent provincial successions and therefore each takes serial rank. Cannonball terrane is the attenuated margin of a greater rock-body the full development of which is to be looked for far to the north in Manitoba and Saskatchewan.

KEYES.

Biotic Significance of Cannonball Fauna. Intercalation of an older sea fauna in the midst of a younger non-marine succession is not nearly so exciting a discovery as recent controversy would have us believe. There is merely again brought to sky the long known irreconcilable differences when objects are viewed in narrow or diverse perspective.

The recognition, in North Dakota, of the Cannonball terrane, with its characteristic thalassic fossils of Cretacic tenor, preceded and succeeded by thick deposits carrying distinctive non-marine forms and extensive land floras of admitted Tertiary affinities, strongly emphasizes the grave dangers of adhering too closely to a single fossil criterion in stratigraphic age determinations.

The Cannonball incident is far from being an anomaly in geology. It is not even a strange or novel occurrence. There is repetition of it time and time again in the Coal Measures of Iowa, Missouri and Kansas. This very feature is one which Barrande so conspicuously paraded long ago in regard to the Cambrian section of Bohemia. With this identical circumstance we are quite familiar at the present day. In view of these facts the only question which really arises is: What criterion are we to follow?

It is not a question of which one we should give precedence to, our floral brethren, our invertebrate brethren, or our brethren attached to vertebrates, but whether we should consider the claims and conclusions of any of them.

Careful analysis of the arguments advanced, *pro* and *con*, at once discloses the fact that in geological age determinations it is not really the rocks which are under consideration but the accidentally contained fossils, which of course is a very different thing. Just as in modern dredging operations in the Gulf of Mexico we find that the echinoderms, for instance, take on older and older aspect the deeper we go, until finally the deepest forms are strictly

Cretacic types, is no sign that we ourselves are actually living in the Cretacic time. Yet, this very thing is what we are asked to admit in the Cannonball controversy.

Implicit reliance upon paleogeographical maps is particularly unfortunate and unconvincing. None of these maps up to the present time are constructed upon a modern physiographic basis. Hence they are largely not only almost incomplete, but, what is more serious, they are usually highly misleading. Their supporting evidence in the case under consideration is necessarily entirely negatory.

On the other hand, the ultra-conservative policy of the United States Geological Survey in geological classification is equally unfortunate. In waiting for the very last word in age determinations laudable advancement is lost sight of. This *laissez faire* method already puts the Federal bureau far behind the times. Normal forward movement is impossible.

To one who has seen the Cannonball terrane only by gas-light its stratigraphic problems seem to present no very great difficulties. Its diastrophic measurement appears relatively easy, and, moreover, harmonizes perfectly the conflicting lines of biotic inference. The Lance formation, of which the Cannonball member has always been regarded as the uppermost division, rests on a marked erosion unconformity.

This stratigraphic break appears to have very large value, as Doctor Cross suggests, instead of small importance, as Professor Schuchert assumes. It seems to have very wide expanse; and to be represented in force on the Rocky Mountain front, where it is known as the Laramian Hiatus, and where a mile-high pile of strata are destroyed. There it marks the undoubted base of the Tertiary succession, a great basal section of which is older than any other Eocene beds that we know on the continent and perhaps in the world.

In such an Eocene sequence it does not seem out of usual order that there should be at some time or other an incursion of marine faunas which would leave their impress along an old temporarily advancing seashore. Settlement of the Cannonball question supplies the basis for all modern terranal classification. It is probable that this will be done eventually upon other than paleontologic principles.

KEYES.

Ancient Salt Lake Cannonball? Campbell makes the observation that "It seems probable that the Cannonball member of the Lance formation is Tertiary in age, and that the Cretacic fauna which occurs it is merely a surviving remnant of an old Cretacic fauna which formerly lived in the open sea, but which as this sea became more and more restricted and eventually enclosed by land preserved its old forms even into Tertiary time."

It is not impossible that the Cannonball conditions were those of a bittern lake instead of those of an open arm of the ocean. All references to the organic remains assume that they form a marine assemblage. That they might just as well constitute a salt lake fauna is supported by many circumstances.

The Fox Hills sea was a vast body of epicontinental waters which finally completely withdrew from the region. In the course of the withdrawal the strictly thalassic forms would naturally soonest disappear. If a considerable land-locked basin remain the littoral types which the thalassic forms removed might persist for a long time, perhaps until the lake entirely dried up. In this way apparently a marine deposit could exist in the midst of freshwater, fluviatile and epirotic beds. Thus all the incongruous biotic difficulties are easily harmonized. Paleogeographic features do not have to be modified. Diastrophic conditions are satisfied. Taxonomic discrepancies are removed. There are no compromises to be made among discordant factors.

Scrutiny of the so-called marine aspects of the Cannonball hypothesis of the existence of a great salt lake seems worthy of fullest consideration.

KEYES.

Northernmost Extension of Marine Eocene Beds in Mississippi Embayment. There recently came into my hands, through the kindness of W. A. Nelson, State Geologist of Tennessee, a specimen collected by Erasmus Haworth six miles north of Campbell, at the northern extremity of the Crowley Ridge, in Dunklin County, Missouri. This specimen is of especial interest since it is a good sized fragment of a one-inch band of gray, somewhat sandy limestone made up almost entirely of the shells of *Ostrea pulaskensis*, Harris.

This shell is a characteristic small ostreid form of the lower Midway section — one that is exceedingly common in Midway out-

crops farther south in the embayment area. The probable extension of the Midway formation to the head of the Mississippi embayment was long suspected, paleontologic evidence that its marine character was preserved to its northernmost limits was heretofore lacking.

In his discussion of the Cretacic-Eocene junction Stephenson states that from central west-Tennessee northward "in Tennessee, Kentucky and southern Illinois, the deposits immediately above the Cretaceous-Eocene contact are of shallow-water origin and so far as known are barren of invertebrates."

West of the present Mississippi River in Arkansas, the Midway Eocene deposits are largely eroded, or covered. A narrow outcrop extends from near Rockport, in Hot Springs County, to Little Rock, in Pulaski County, and the northernmost previously known occurrence of *Ostrea pulaskensis* is in Pulaski County.

Northeast of Little Rock, Arkansas, Midway deposits are recognized by Stephenson and Crider, in wells and surface outcrops at Jacksonville, Pulaski County, near Cabot, in Lonoke County, at Beebe, White County, and from Bradford, in White County to Depart Creek, in southern Independence County.

This occurrence of *Ostrea pulaskensis* in southeastern Missouri is nearly 100 miles northeast of the most northerly outcrop of Midway beds recognized in Arkansas, and nearly twice that distance from the most northerly recorded occurrence of this species in Pulaski County, Arkansas.

The circumstance shows that not only did marine faunas flourish near the very head of the Midway Mississippi Gulf, but also that the Midway sea transgressed rapidly over the nearly base-levelled Late Cretacic land surface that had been exposed during the long time interval now marked by the Cretacic-Eocene junction line.

BERRY.

World's Scarcest Crinoids. Few scientists have the sense of humor developed so strongly as did the late Charles Wachsmuth; and few are so fond of quiet practical joking. When he was finishing up that monumental joint effort on the *Crinoidea Camerata* and was beginning to survey the field for the next installment of crinoidal inquiry he one day petulantly threw down some Flexibiliate forms which he was examining and remarked that he

would just leave that group for Springer to wrestle with, even if it took the latter twenty-five years. And sure enough, when his colleague who was then away returned, he gravely turned all the collection over to him for study and report. But Wachsmuth did not fully know his long time colleague and most intimate friend, nor his dogged perseverance. He little suspected in his life time that the odd, scragly little assemblage of fossils which he so readily spurned would in the course of a quarter of a century grow into a great company, the account of which would form a sumptuous installment equalling in every way the one which he had just helped to complete.

Mr. Springer thus outlines his latest effort: "It is a fresh illustration of the growth of knowledge that the division of the Crinoidea which forms the subject of the present memoir was not known at all to the earlier systematic writers who treated of the Class; neither to J. S. Miller, with whose epoch-making monograph the systematic study of the crinoids as a group began nearly a century ago, nor to Johannes Mueller whose masterly researches upon the anatomy of the Echinoderms twenty years later, laid the foundations for future investigations upon their structure. The magnitude of the group as now understood is shown by the size of this treatise — and the progress above alluded to is further exemplified by the manner in which the subject has expanded under my hands.

"When I began the study of *Flexibilia*, after the death of Wachsmuth, in 1896, it was part of a more ambitious plan to work up the two groups remaining after the *Camerata*; and of these it was supposed that the present group would be relatively a minor undertaking. I estimated that twenty-five plates would include all the necessary illustrations, and that these, with the text, would fall readily within the compass of a single volume. All the known material in this group in the museums of the world at that time did not occupy one-fourth of the space that is now required for the specimens of my own collection.

Except for a few species, the *Flexibilia* are the rarest of all the fossil crinoids, some forms being represented by a single specimen, and most of them by only a few. It was my early perception of the inadequacy of material, of the necessity of making further collections, and of examining as far as possible the types and other

specimens from all sources, that has in part caused the long delay in the preparation and publication of this work. For the greater part of the delay, however, has been due to the desultory character of my studies, arising from causes not within my control. The insistent demands of an exacting profession, and the claims of business affairs which absorbed the major part of my time, caused frequent and often long breaks in the prosecution of the work, the total of which must be measured in years.

"These interruptions, however, have not been without their compensating advantages; for during all this time the acquisition of new material, chiefly through the medium of collectors in the field, has been steadily going on, resulting in important additions to our knowledge of this group. And the broader grasp of the subject consequent upon this increase of knowledge has enabled me to place on a firmer basis certain family divisions, which would have been left in an unsatisfactory condition if I had published my results a few years ago.

"I think it only fair to observe further, by way of personal allusion, that I have labored under the disadvantage of a lack of practical zoological training, which compels me to limit my treatment of the subject chiefly to the presentation of the facts from a systematist's standpoint, without venturing too far into the field of evolutionary interpretation. This I prefer to leave to others who are better qualified to undertake it, and it is my hope that this contribution to the sum of knowledge of these organisms may be of some service to those who engage in more general discussions.

"It was evident to me at the outset that the plan of restricting the detailed investigation of this group to its American representatives, as was done in the treatise on the Camerata by Wachsmuth and myself, was unsatisfactory. I have therefore endeavored to include in this work all known species of *Flexibilia*."

KEYES.

DYNAMICAL GEOLOGY

Origin of Desert Ranges of Mexico. Desert geology acquaints us with many new ways of regarding geological phenomena. Their consideration from the new angle throws a quite different light upon some of the relief features which were long ago thought to be fully and easily explained. A welcome addition to our knowledge of the geology of Mexico is the brief communication presented at the recent Wilkes-Barre meeting of the American Institute of Mining and Metallurgical Engineers by Charles Laurence Baker, on the "Catorce District in the State of San Luis Potosi."

Catorce is an old and important silver camp of Mexico, concerning which we have had too little information. Mr. Baker does not fall into the usual scholastic blunder of describing the mountain range at Catorce as a ridge due to block-faulting, although the Sierra de Catorce is one of the desert ranges which, at least on the American side of the border, has been sweepingly condemned to this origin, regardless of their individual geological structure.

Mr. Baker observantly explains: "As is usual in desert regions, the heavy-bedded limestones are the most resistant to erosive forces. Folded into anticlines they form the mountain ranges; and the intermountane valleys and basins, originally carved out of less resistant shales, clays, marls and sandstones, are covered by debris, forming a mantle with surface gradually rising towards the hills." A lucid, true and appreciative description of the desert ranges of northern Mexico, as contrasted with the usual twaddle about block-ridges, and the chatter of horst and graben.

SPURR.

Minimum Span of Isostatic Effect. When the conception of isostatic compensation, that most brilliant geological hypothesis of our century, saw birth amidst the isolated desert ranges of the

Great Basin it was fancied that so responsive was the earth's crust to local loading and unloading through erosion the smallest mountain ridge or a single mountain was quickly and profoundly affected. The short, lofty, apparently tilted mountains were looked upon as huge fault-blocks floating upon the soft interior of the earth mobile almost as ice-cakes in a river at time of spring-break-up. This view of isostasy's warmest supporters proved to be extreme and illusive.

As, a generation later, this hypothesis began to undergo critical test for its verity it was found that the first extreme application was very far from holding. For such mountain masses as the Great Basin presented there appeared to be no marked recent movement discernible. Instead of being orographic blocks lately faulted and tilted the desert ranges proved to be really mountains of erosion. Their supposed fault-scarps were girdling effects of desert abrasion. No fault-lines characterized the mountain bases. The tectonics were relatively ancient. Whenever fault-line was discerned it was far out on the plain. Isostatic compensation surely did not obtain in the case of narrow single mountain ranges.

On a larger scale the cordillera of the Rocky Mountains is recently the theme of isostatic measurement by means strictly geologic. This tract seems to be exceptionally well suited to determine the span of isostasy. From the earliest geologic times it undergoes continuous diastrophic oscillation. The area is one which has been repeatedly uplifted. It is one which suffers again and again extensive planation. With it orographic change is never so great as to obliterate entirely the salient features of the successive movements. Later upraisings and down-sinkings are especially well marked. The geologic record of events is unusually full.

Since the close of the Paleozoic era great planation periods mark Comanchan, Laramian, Miocene and Recent times. Comanchan peneplanation is particularly wide spread. Singularly, the Jurassic ancestral Rockies do not appear to have grown higher as their substance wasted away, as isostasy demands. They allow themselves to be worn down to the very level of the sea. After reaching that level, when all their positive volume had vanished, the tract which the mountains occupied proceeds to sink. At any rate depression is active until the old sea-level plain attains a

position more than two miles beneath the surface of the ocean. Despite the removal of its former great positive load the net results of regional vertical movement is notably negative.

Procedure is the same in all four orographic cycles. Removal of a load through erosion is not accompanied by further upraising. Rolling the oceanic waters over the tract follows regional unloading. Maximum sedimental loading of the area is immediately succeeded by great uplifting. Every stage is met by phenomena diametrically opposed to those which isostasy logically and vitally demands. The isostatic hypothesis fails utterly to sustain itself before such an array of critical geological testimony. Geologically the crustal span of isostatic compensation seems not to hold for a cordilleran tract so wide as the state of Colorado.

Isostatic hypothesis receives its strongest support from mathematics, albeit strictly theoretical. Exceeding fine certainly grinds the mill of mathematics. Yet, what comes out of such mill depends very largely upon what is fed into it. So that, concerning earth problems, there is, perhaps, often little to choose between rough guess of geologist and most painstaking calculation of mathematician.

In view of recent comprehensive observation in the Rockies, bearing critically, it seems, upon the very foundations of isostasy, it appears that the mathematical equations will have to be radically modified in order properly to satisfy the necessary consequences lately derived directly from the field, or else they will have to be given up as purely fictitious.

It is not at all strange that the Rocky Cordillera should on the one hand, so strongly belie the verity of isostasy, and, on the other hand, should point so conclusively to the strictly telluric nature of its genesis, and indeed of orogeny in general.

Referring characteristic mountain tectonics, with which we are best acquainted, to the secular diminution in rate of the earth's rotation, all the principal orographic features are readily and satisfactorily reproduced in the laboratory. At best, then, mountain genesis appears to be merely feeble expression of the larger telluric properties of our globe.

KEYES.

Changing Sphericity of Our Earth. Strictly global aspects of our planet seldom appeal to earth students to the full extent which

they often merit. This neglect is not so much simple oversight, as it is apparent hopelessness of possible analysis due partly, perhaps, to the fact that so many of the factors involved are naturally expected to find little direct expression either geologically or geographically. Then, too, the evaluation of such changes are doubtless so relatively inconsequential and uncertain as to be lost among those of more striking local features. Visible physiognomy of sphericity changes would be anticipated as about the very last thing to be geologically recorded. Small wonder then that they are so little liable to demand serious consideration. Yet, we seemingly recently attain something even on this score.

In a purely theoretically way the keen Newton early attacked the sphericity problem. Between the date, about 1680, when he demonstrated mathematically that our rotating globe was appreciably oblate in form and that the length of its axis was twenty-one miles less than the equatorial diameter, and the time when actual measurements were made by geodesists proving the conception, a century and a half elapsed, so slow was the clue taken up. So amazingly accurate were the British philosopher's calculations that modern results make only small rectification necessary.

In past geological ages the earth's oblateness was not always the same as it is today. Fifty-seven millions of years back, or about at the close of the Paleozoic time perhaps, when the sidereal day was only about one-fourth as long as at present, or of six and three-fourths hours duration, the difference between the axial and equatorial diameter must have been very much greater than now. The direct deformat effect of a diminishing rate of rotation would be a rising of the polar regions and a sinking of equator belt. Such polar expansion would ordinarily probably leave little or no geological impress because of the slowness with which the change took place and its masking by more conspicuous epicene operations.

Bulging of the polar tracts would have another obscure effect of large dimensions. There would be a tendency for the crust to stretch from time to time. Such secular stretching would necessarily be so gradual and so likely to be overshadowed by other crustal movements that it could hardly be expected to be

measurable under usual conditions and with the ordinary means of evaluation at our command.

Only under especially favorable circumstances could first clues to such phenomena become discernible. Only on some great plains tract which had long been free from the effects of mountain making activities could proper conditions of measurement obtain. Even with the most refined instruments, with comparisons of results of observations extending over a period outspanning a human life would appreciable returns appear. The physical difficulties of solution seem so great as to prohibit any systematic initiation of proper measurements even if it were desired to seek only rough estimates.

Actual instrumental measurements of a stretching crust recently come from an unexpected quarter. It may be that they are the very proofs so long sought. Until the same are shown to be something to the contrary it may be safely assumed that they afford possible and concrete evidences of polar bulging and changing sphericity demanded by a constantly diminishing rate of the earth's rotation. This desideratum hails from Denmark.

In the recent application of the refined Jäderin apparatus to the Copenhagen base-line, upon which the triangulation of Denmark fundamentally depends, the Coast and Geodetic Survey of that country found that the initial measurement was 51 millimeters more than the figures of the old determination made by Schumacher in 1838, to which all calculations had been so long referred. So disconcerting was this discrepancy that the Potsdam auxiliary base was measured and compared no less than ten times. A new Danish base was also established and its length adjusted to the old Copenhagen base. As a result the figures of 1838 were accepted as essentially correct; and the length of the Copenhagen base-line was proved to have appreciably lengthened during a specified period of about three-quarters of a century.¹

The feature of great interest in the present connection is the nearness of agreement between the observed amount of lengthening of a finely measured line such as the Copenhagen base in 80 years, and the theoretically calculated stretching of a segment

¹ Den Danske Gradmaaling, No. 15, Nye Basismaalinger i Danmark, 88 pp., Copenhagen, 1916

of the earth's crust in the latitude of Denmark in the same period, having the secular retardation of the earth's rotation as a controlling factor. Rough guessing seems to indicate close agreement. The amount of stretching now appears mainly ascribable to secular polar bulging.

Before implicit acceptance at its face value of stretching of the Copenhagen base line as evidence of polar expansion effects the phenomenon will have to be geologically analyzed with special reference to the possibility of telluric stress release along circumscribed belts or along fault lines. We gather something of the *modus operandi* from consideration of the rifting which produced the San Francisco earthquake of 1906. Below Point Reyes where the movement was greatest and amounted to ten or twelve feet, the horizontal component is indicated by the off-setting effects of fences, roads, and even buildings. In order to determine possible future movements however slight, the University engineering corps planted stone monuments at certain points on either side of the rift.

The San Francisco movement is in mountainous country; and all lines in mountainous tracts obviously have to be excluded from polar bulging calculations. Only great plains situations are entertaining. In the case of the Copenhagen base-line possibility of its crossing not an actual rupture but a line of crustal strain has yet to be excluded.

The stretching of the Danish base-line is worthy of most careful inspection with regard to its telluric affinities.

KEYES.

Composite Nature of Rock Mass-movement. Under its environmental forces the solid earth, the popular symbol of strength and permanency, proves weak and incompetent. The crust bends, crumples, breaks, and mashes. In an engineering sense the lithosphere fails; in considerable part it now consists of structural ruins.

Movements of rock-masses within the zone accessible to observation are accomplished by fracture and flowage. These processes may be distinct and separate, or so inter-related as to make definition difficult. The zones of movement are many; their positions and attitudes diverse. In general, they indicate shearing or grind-

ing movements between rock-masses, accomplished both by fracture and flowage, and caused by stresses inclined to the planes of motion. This conception is taken to afford the best initial basis for the interpretation and correlation of the observed rock structures.

There is no certain evidence of increase or decrease of movement towards the bottom of the zone. Beyond a shallow surface zone, there is no certain evidence of increase of rock-flowage and decrease of rock-fracture with depth. There is no certain evidence that rock-flowage means greater weakness than rock-fracture. There is no certain evidence in rock-flowage that pressures are dominantly hydrostatic or dominantly those of competent solid bodies.

In the zone below our range of observation movements are known to occur, but their nature and distribution are subjects of varied hypotheses based upon a few known conditions. Much of the sharper diastrophism seems to be confined to a thin surficial zone. Deeper movements, of a more massive type, periodic, and possibly slower, seem to be implied by the relative movement of the great earth segments as represented by the continents and ocean basins. Their depths are unknown.

Most of the current hypotheses agree in assuming a single mobile zone in which rocks move dominantly by flowage. However, the basic requirements of reasonable hypothesis may be met equally well by a conception of movement much like that of the zone of observation. This does not require or postulate the conception of the existence of any single mobile zone, or zone of slipping, or zone of flowage, or of an asthenosphere. It premises movement irregularly distributed in many zones, with any inclination, and accomplished by both fracture and flowage as far below the surface movement extends — always remembering that some of the structures geologically described as fractures, may be expressions of mass-movements of the kind defined as flow in experimental results.

With depth, conditions of temperature and pressure and vulcanism become more intense, but it remains to be shown that conjoint action results in a uniform environment and even if it does, that this condition is not upset by what might be called a heterogeneity of the time-factor as represented by different rates of deformation.

If homogeneous environmental and time conditions be premised, it is yet to be shown that these are sufficient to overcome the heterogeneity of the physical properties of the rocks and to cause homogeneous behavior through any considerable zone. It is not even certain that they may not fix and accentuate the heterogeneous properties of rocks. Certainly in the zone of observation there is comparatively slight evidence of their efficacy in causing more uniform deformation with depth.

LEITH.

*Discovery of Gilbert's Star.*¹ Wide interest was aroused several years ago when G. K. Gilbert announced that when he visited Arizona a short time before he examined a large crater-like depression, hollowed out below the level of the surrounding plain, and about which large numbers of metallic iron fragments had been found. Coon Butte the place was designated by the dwellers about. Regarding the origin of this wonderful hole in the ground the hypothesis was advanced that it was due to a huge meteor striking the earth.

The features of this locality include three things of unusual character which require explanation: (1) the deep hole ringed all around with a ridge composed of non-volcanic materials; (2) the abundance of scattered meteoric iron; and (3) the association of the two phenomena.

The features displayed are not nearly so unusual as Gilbert would have us believe. Meteoric irons are especially abundant in the dry scant soils of the desert. It is not probable that they are really any more numerous in this locality than they are in other equal areas of the earth's surface, but simply that they are more easily found. The strong desert winds keep the soils removed and the rock-floor of the plains swept clean so that the heavy iron fragments are left behind exposed to plain view. So abundant are the fragments that the Indians of the region make it a regular business to collect these irons to sell as curiosities to tourists on the trans-continental railroad trains. As a matter of fact meteoric irons are far more abundant a hundred miles from Coon Butte than they are around the depression.

¹ Under this title a paper from which the present note is abridged, was read before the Geological Society of America at the Ottawa Meeting. In publishing it in Volume xvii of the Bulletin the editor asked permission to change the title to "Volcanic Craters in the Southwest," which was accordingly granted.

This hole in the plain is not a novelty by any means. The location of Coon Butte is well within the field of the San Francisco Mountains, one of the large volcanic areas of the continent. Within plain sight of the Butte are 400 similar phenomena most of them associated with small ash-cones. A few miles east of the Butte is another hole in the plains known as the Zuni Salt Lake. It is an exact replica of the hole Gilbert saw. Like the latter it has all around it a wall of non-volcanic materials.

But Zuni crater presents some tell-tale features which the Coon Butte does not show. On the floor of the excavation rise two small ash-cones, about 300 feet high, one with as perfect a crater as that possessed by Vesuvius. Certainly the last mentioned hole is not the effect of impact of a falling star. If it is, one may well look askance at the 400 other similar holes in the vicinity. Coon Butte is not by any means a unique phenomenon even in its own neighborhood.

The well known mathematical law of probabilities is strongly against the supposition that a giant meteor would especially select a restricted volcanic field on which to alight, or that it would thus attempt to disguise its origin and identity in a finest company of volcanic craters in all the world. The Falling Star hypothesis clearly needs further elucidation and substantiation.

KEYES.

Major Telluric Stresses Initiated by Diminishing Rate of Earth's Rotation. For quantitative determination of the larger effects of diastrophic movements the variant centrifugal and tangential pressures induced by changes in rate of the earth's revolution seem to offer most promising results. Recent curious experiments in geotectonics appear to indicate in no mistakable terms that the grander relief features of our globe are all readily reducible to the same, simple tangential forces.

In view of the fact that the immediate cause of the great earth-wrinkles is usually approached from the astronomical side the contractional hypothesis takes form on the assumption of a cooling globe. It is premised that the earth passes through the same process as does the desiccating apple. For a period of three centuries this idea widely prevails. Beginning with Descartes

and ending with Suess the contractional hypothesis finds many adherents.

Although the expansion theory of Hutton, the theory of isostasy of Dutton, the theory of extensive crust-slide of Reyer, and the theory of upheaval of Rothpletz cannot be expected completely to replace the contractional hypothesis they especially serve to call attention to some of its shortcomings. There are still graver objections than those mentioned to the contractional theory of orographic and continental genesis. These appear as direct results of practical experiment in the laboratory.

In laboratory experimentation on curved prisms, like sectors of the earth, with bands to take the place of gravitational control, and under conditions analogous to retardation of the earth's rotation, there is reproduced to a nicety all of those larger structural features of the crust such as the oceanic basins, the continental arches, the cordilleran corrugations, and the orographic foldings. Effects of tangential creeping which many mountain structures display thus appear to be not due necessarily to results of the earth's contraction, but to direct cumulative release arising from secular retardation of the earth's rotation.

On this new basis, with the force and rate of retardation, and the amount of crustal shortening capable of exact expression by mathematical equation, a ready means is provided for realizing not only something of Élie de Beaumont's fantastic dream of orographic symmetry, but for gauging in units of human time the age of every mountain uplift, for determining within very narrow limits in like terms the periodicity of every diastrophic movement, and for evaluating in years not only the span of every era, period, epoch and stage of stratigraphic record since life began, but stratigraphic chronology long antedating the life record itself.

KEYES.

Continental Dynamics. There is a definition of continent other than the one usually accepted. In it the hydrosphere is left out of consideration and the definition is no longer a geographic one. The facial expression of the globe is then an effect of our earth with a land area only. A condition is premised analogous to that of our waterless moon.

Genetically the oceans serve merely to obscure the larger tec-



VOLCANIC CONE OF ELBURZ—HIGHEST MOUNTAIN OF EUROPE

tonic significance of relief expression. Relation of sea and land is made casual and essential instead of merely accidental and trivial. Consequently the geographic definition of continent is really meaningless.

Instead of distinguishing between continental platforms and oceanic depressions, a circumstance imposed by an unweening importance attached to the presence of the sea — a notion handed down from time immemorial — the proper discrimination to be made is between the continental ridges of the continental borders and the intervening lowlands whether above the level of the waters in the continental interiors or beneath sea-level as in the oceanic areas. On this basis the tracts which we are accustomed to designate the oceanic depressions and the sea-level interiors of the continents are arranged in the same taxonomic category. Consideration of any such datum plane as sea-level may be with full propriety entirely neglected. The meridional disposition of the continents thus comes to be readjusted as relatively narrow orographic ridges in place of broad basin-shaped plateaus.

The tectonic consideration of a waterless earth casts a new light upon the schematic form of our globe. In its logical consequences the contractional hypothesis finds expression in such figments of the imagination as the *reseau pentagonal* of Élie de Beaumont, and the tetrahedral globe of Lothian Green. To be sure the form known as the tetrahedron is of all geometric solids the one form which possesses the least volume in comparison with a given surface area, while the sphere contains the greatest bulk within the same surface; yet the collapse of the latter is not necessarily any such crystallographic shape as that indicated by the former.

In the present state of our knowledge and schematic form of our earth is largely conjectural. However, it is suggested lately that in the case of a collapsing spheroid the initial tendency towards a faceted form would probably not be directly in the line of any limiting shape as a four-sided figure, but towards something intermediate, between a limiting shape and the most general form, or a figure having twelve or twenty-four faces. That the rhombic dodecahedron is possibly the real plan, if there be any, although having in nature curved surfaces, seems to be borne out by the trend of the chief mountain ranges of the world, and

by the situation of the main volcanic activities at the sharp solid angles, or the points where each set of three faces intersect.

Viewed, then, in their larger or telluric relations the continents are probably best regarded not as broad basins with upturned rims but as somewhat irregular, interrupted meridionally disposed ridges. These ribs appear to be the directly traceable in their genesis to release of cumulative tension that depends upon the secular retardation of the earth's rotation.

KEYES.

Geological Directrix of Isostasy. Seemingly insuperable difficulties which are encountered since geodists took up mathematically the solution of isostatic compensation in the earth's crust now appear to be not nearly so irreconcilable geologically as not so very long ago was quite generally supposed. The differences prove to be mainly one of viewpoint.

As the prospects of the outdoor geologist and of the indoor mathematician become hopelessly confused it is discovered that there are actually under consideration two entirely distinct and essentially unrelated things. The one resolves itself into simple hydrostatics, which is, of course, wholly independent of any geological data which may be adduced; while the other passes into impossible tectonics. Recognizing the fact that the isostasy of Hayforth and Bowie is not the isostasy of Dutton and Gilbert, Becker and Davis, among others, ascribe the isostatic incongruities to tangential pressure and telluric crushing effects, which are clearly not isostatic properties at all.

Geologically prime interest in the isostatic theory concerns not so much its verity or falsity as it does the minimum crustal arc over which it is appreciably effective in mountain construction. Such compensation is neither so far-reaching as mathematicians proclaim nor so perfect and delicate as geologists sometimes argue. When from recent gravity measurements Gilbert shows that some large mountain tracts are at least partially self-sustaining latest observations in western America seem amply to testify.

It is not so surprising that these recent determinations should appear to demonstrate conclusively that isostatic compensation does not obtain in the case of single mountain ridges, as the isolated desert ranges of the Great Basin. But when it appears to

fail even for great cordilleran tracts the width of the State of Colorado a pointed question at once arises as to what is really the geological datum plane from which calculations are started and to which conclusions are referred.

Both geodesist and geologist starts his problem at sea-level. While the first mentioned throughout his calculations retains that datum plane, the other soon wanders away from it and gives first reference place to a plane which he can clearly follow in the field. In the last analysis the geological directrix turns out to be some regional erosion plain of the first order. Among more recent instances it is a peneplain; among the more ancient cases it is a great plane of unconformity. The relation to sea-level of such plane more or less extensively deformed though it subsequently becomes, gives the geologist his chief clue to the evaluation of diastrophic movement. All unconsciously, perhaps, he extends his diastrophic estimates to isostatic problems. With his engineering bias of sedimental loading and erosional unloading of local tracts of the earth's crust he naturally and quickly reaches some old peneplain as a basis of his figuring.

Starting at sea-level, or more properly at a hypothetical level about two miles below, the geodetic calculations treat the datum as a constant and the crust as an amorphous, geometric solid of great and indefinite thickness. With the same starting point the peneplain becomes a variable factor; and the crustal prism possesses a straticulate structure susceptible of recording the slightest deformations and the smallest vertical movements. The two datum planes not only do not always lie together, but they are usually quite distinct, and soon may become miles apart. Their relative positions may vary between zero and infinity. Small wonder, then, that the geodetic and geologic conclusions are apparently so contradictory.

Geodetic conception has the crustal prism, a rigid, structureless block, 70 miles or so in thickness, floating upon the mobile interior. Geology considers the same prism with varying thickness, only five or six miles in vertical measurement, and having the bottom of the old rock-section flowed off to form schists; the part of the section above the old peneplain being replaced by newer sediments of specific gravity not so very different from that of the more ancient rocks. When, owing to a return swing of the dias-

trophic movement, there is regional upraising the upper part of the local prism which is elevated and eroded until the old peneplain again reaches sky and a level perhaps of two or three miles above sea-level, the lower section appears as a schistose mass. Such seems to be the very thing which happens in the Rocky Mountains, the Piedmont Plateau and many other places.

According to mathematical equation established or assumed the depth to which crustal compensation acts is about seventy miles. Geologic interest does not extend nearly so far. At a depth of five or six miles all rocks are crushed and facile rock-flowage is attained. The mobility of a rock-mass and the recrystallization along movement planes such as develops schistosity, surely is as perfect at six or seven miles as it is at seventy, or seven hundred miles.

This very thing appears to have transpired in some of the cordilleran regions. In the Piedmont Plateau Paleozoic sedimentation takes place on a sinking tract until 30,00 to 40,000 feet of deposits accumulate. The great pre-Cambrian section of sediments, the Cambric, Orovicic, and Siluric piles are carried down quite into the zone of rock-flowage. When they return to sky again the old Paleozoics appear as schists, which until very lately are supposed to be Azoic in age and among the most ancient rocks on our continent.

In the southern Rockies the course of events is similar, only that it is repeated again and again. Here, curiously, the geologic conclusion on diastrophic movements is that the latter directly contradict at every stage the essential demands of isostasy. This, however, is not necessarily so. Considering the zone of rock fracture as a geometric solid without reference to its geologic structure, it makes little difference to what depth a given peneplain be depressed. Even with two to four miles of sediments resting upon this old land surface a rock prism over each particular area remains isostatically balanced. In a depressed block a considerable part beneath the level of the buried peneplain long since flows off and loses its sedimentary identity. In both cases the geodetic datum is a fixed one. Not so with the geologic directrix; for the latter is a variable quantity, and at no time or point is it coincident with sea-level.

On the other hand an upraised peneplain is followed closely

by an uprising of the upper limit of the former zone of rock-flow. In this instance the structureless geometric block and the stratified prism really coincide. Yet, the mathematical datum still remains at sea-level and the lower portion of the geometric block really passes into the zone of previous rock-flowage.

No mountainous tract, perhaps, is so especially well adapted for test of diastrophic movement, or for determination of the span of isostatic adjustment as the Rocky Mountains. This noble cordillera occupies a broad belt, which, from the earliest geological times, has undergone almost continuous oscillation. The area is one which has been repeatedly uplifted. It is one which has suffered again and again extensive planation. Its orographic change is never so great as entirely to obliterate the salient features of the different movements. Its later upraisings and down-sinkings are exceptionally well marked. The geologic record of events is unusually full and clear.

Although from very earliest eons the southern Rocky Mountain tract experiences repeated upliftings those taking place since the close of Paleozoic times are most remarkable. The latter are four in number. There are a like number of total effacements. Each of the ancestral Rockies appears to have been no less majestic than are the Rocky Mountains of today. Each of the old levelings is down to the surface of the sea. Peneplanation in each instance is as perfect as is probably ever attained. Great planation epochs mark Comanchan, Laramian, Miocene and Recent times.

Both local and regional loading and unloading through sedimentation and erosion are strictly problems in mechanics. According to the fundamental premise of the isostatic hypothesis mountains continually rise because of the removal of their summits by erosion, thus lightening the local crustal load. On the other hand, depending upon the argument of the astronomer Herschel, areas of sedimentation are tracts of notable insinking of the crust.

Singularly, the Jurassic ancestral Rockies appear not to have grown higher as their substance wasted away. Without sign of struggle they allow themselves to be worn down to the very level of the sea. After reaching sea-level, when all their positive volume vanishes, the tract which the Rocky Mountains once occu-

pied, instead of continuing to rise, or at least to remain stationary, proceeds to sink. Indeed, sinking of this area is in evidence long before base-level is reached. At any rate, depression is active until the old erosion plane attains a level of more than two miles below that of the surface of the ocean. Where once lofty mountains pierce the sky marine sediments 10,000 feet in thickness accumulate. Despite the removal of its former great load the net result of the regional vertical movement is notably negative.

Insinking of the earth's crust in this region appears not to have taken place *pari passu* with extensive transference of sediments. Cretacic depression manifestly initiates itself long prior to the beginning of sedimental loading, perhaps even before the mountains are completely demolished. Then, curiously enough, so soon as the enormous loading is finished the region uplifts again into lofty ranges as towering possibly as any which appear before or since. Instead of the sequence of events satisfying the isostatic equations the very reverse is true all through. Thus, crustal down-sinking succeeds to regional unloading; and orogenic upraising goes on under maximum load. Surely some orographic force other than the isostatic one is at work.

In all four of these orogenic cycles the procedure is the same. Removal of load is not accompanied by further upraising. Rolling of oceanic waters over the tract follows regional unloading. Maximum areal loading is succeeded by great uplifting. Every stage meets with phenomena diametrically opposed to that which isostasy logically and vitally postulates.

A noteworthy shortcoming of the mathematical theory of isostasy is the circumstance that no distinction is made between the physical behavior under telluric stress of the thin crustal shell of rock-fracture and of the thick interior mass of rock-flow. In view of the fact that any depression or insinking of the crust due to sedimental loading occasions removal of an equal thickness from the bottom of the affected prism through direct flowage it may be questioned whether any such rock-prism appreciably changes its specific gravity, so slightly in this respect do rocks in the zone of fracture differ from one another. What really, then, is mistaken for floatage phenomena is adjustment in the earth's crust of the effects of centrifugal force arising out of a retardation of rotation of the geoid.

Isostasy of the geologist and isostasy of the mathematician are not, then, by any means the same thing. This is the basic reason why in the Rocky Cordilleran Region, as already emphasized, the geologic observations on isostasy are so directly out of harmony with geodetic demands. In following the only available datum plane from which to measure the amount of alleged isostatic flotation the geologist at once branches off from the fixed level of geodesy. Each group of conclusions are thus necessarily antithetical. The geological directrix of isostasy resolves itself into an old erosional plain, or peneplain as the geomorphologists are pleased to call it. In all considerations of isostatic measurement this distinction is obviously fundamental. In all the elaborate calculations of mathematician the latter does not really touch the conception of geologist. Although the geological directrix of isostasy is the one factor of all upon which geodetic hypothesis depends yet it appears to be without representation in mathematical equation.

KEYES.

Geotectonic Economy of Thrust-faulting. Critical analysis of the larger geologic relations of normal and reversed faults emphasizes the fact that it is illogical to consider them together, as if they belonged genetically to the same order of structural phenomena. The one being merely limited crustal adjustment through gravity presents marked contrast to the other which appears to be the direct expression of cosmical stress, and therefore in so far as the earth is concerned, of universal causes.

That thrust-faults occur more frequently in the hard or brittle formations than elsewhere, that they are mainly confined to the very old rocks rather than to the younger terranes, that they become more prominent, more important and more numerous in the bottom of the earth's crust, or zone of rock-fracture, than near the top, and that under ordinary conditions they rarely reach sky except through chance exposure by profound local erosion, are generalizations of great significance but attract small notice and are little associated genetically.

This broad or universal cause appears to be found in the retardation of the earth's rotation, producing repeated and cumulative displacement of the earth's radius of molar repose, or radial line of no strain.

KEYES.

Erosional Agencies under Varient Climatic Stimuli. It is the custom to treat the subject of general land-sculpturing not only independently of climatic considerations, but as if the molding of all landscape features were controlled by the same geologic laws.

The fertility of suggestion growing out of the novel conception of a definite cycle of development through which all land-forms must pass tends to exaggerate the evolutionary aspects of the theme at the expense of the genetic means by which the physiographic changes are accomplished. Even the latest and most authoritative treatise on physical geography premises the same derivation of physiognomy for the profoundly glaciated Swiss Alps and the arid High Plateaus of western America, for the forest-clad Appalachians and the bare South African veldt, and for the jungle-matted eastern Andes and the desert Australian interior. Ordinary stream corrosion is made to account for all. Rain thus comes to be regarded as the universal and sole graving-tool in landscape sculpture.

These diverse and incongruous phenomena may be viewed from a quite different angle. The relative efficiencies of the different erosional agencies may be quantitatively measured under variant climatic stimuli. It may be shown that under the variant favorable conditions imposed by aridity the wind, for example, may become a general erosive power in every way comparable to that of rain, wave or river. Circumstances may be pointed out under which eolic activity assumes an erosional ascendancy over all other forms of gradation. Some of the most characteristic geologic processes and most typical geographic products may be described in regions where the wind displays its maximum efficiency as a general erosional agent.

Most descriptions of the geologic work of the winds treat the geomorphic effects presented in moist and in dry climates together. There are grave objections to this practice. Under such genetically diverse conditions the resultant phenomena should be separately considered and strongly contrasted. In humid lands the erosive activity of the wind is all but completely dominated by that of the rain. Although this is the phase of the eolation which is really most trivial it is the one which receives usually main consideration.

KEYES.



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ISOSTATIC THEORY; AND APPLIED GEOLOGY

By CHARLES KEYES

Had the Duttonian conception of buoyant mountains found even partial substantiation in those shadowy desert ranges of the Great Basin the whole future of mining might have been fundamentally changed. A few imperfect impressions in place of some mature reflections prostrated the hugest mining project of the ages. That Isostasy should be the end of Cyaniding Science needed only the movement of a little dust, and an unseasonable gust of wind sufficed for abruptly ending a dream of supremest avarice.

In the past decade or so determination of the prevailing occurrence of ore-veins in the desert ranges of western America proves to have a curiously critical bearing upon the verity of that most brilliant geological musing of our century, known as the theory of isostasy. It is a wholly unexpected circumstance that such a practical activity as mining should put such abtruse speculation to its severest test. So diverse are the two fields apparently that any direct association between them seems entirely out of question. Yet, as in the case of so many other branches of science, it is not always from the nearest basic principles that strongest support is forthcoming. First quantitative evaluation of the hypothesis through strictly mining figures is worthy of close examination.

As is well known the hypothesis of isostatic compensation in the earth's crust is reputed originally to find its best surface expression

in the so-called block-mountains of the Great Basin of Utah and Nevada. In fine, these mountains are regarded as huge fault-blocks floating on the soft interior of the earth much after the fashion of jammed ice cakes in a river at time of the Spring break-up.

One does not gather from the writings of Dutton, Gilbert, Powell, Russell, or others of the extreme government experts and advocates of isostasy, that the basis of the hypothesis is not really so much the general impressions gained on the borders of the Great Basin as it is the revival of an all but forgotten idea first advanced by the English geologist Babbage and the astronomer Hershel, a hundred years ago, wherein it was postulated an in-sinking of the earth's crust over areas of accumulating sediment.

Now Captain Dutton, overpowered by the sudden realization of the tremendous volume of the erosion so clearly displayed on every hand in and about the High Plateaus of Utah and attracted strongly to Gilbert's then recently proposed fault-block explanation of Basin Range structure, endeavors to reverse the long antiquated suggestion of Hershel, and argues for a rising of those tracts over which depletion goes on vigorously. Therefore, it is urged that the more the mountain tops are removed the higher the elevations become.

According to the Gilbertian conception of Basin-Range structure, and the Duttonian idea of isostasy the fault-planes assumed as bounding longitudinally the mountain blocks should be lines of profound vertical displacement. These major ruptures should be the very ones of all others to reach down into the zone of plasticity, or rock-flowage. They should be belts along which there is release of pressure sufficient to enable deep-seated molten magmas to reach sky. They should be gashes along which mineralizing effects should be prominently and especially developed. On these narrow bands, also, chief ore-bodies should be situated. On these filled crevices principal mines should be opened. Their position should be marked by rows of shaft houses at the foot of every desert range. In reality none of these features seem to obtain. What is expected and what is actually found are notably contradictory. Mining operations are rarely discovered on the piedmont scarp where plain meets mountain often as sharply as the strand-line of the ocean. Discrepancy between fancy and fact demands rigid inquiry. Should the fault-block idea of Basin

Range tectonics prevail and the conception of isostatic compensation prove valid for crustal segments so small as single mountain ridges the greatest aid ever devised for advancing ore exploration would become a practical reality.

European scientists were never prone to view the speculative conclusions of the Powellian coterie of government geologists with the same complacency as Americans. Among the latter there appeared to be few to question. I happened to be one of these. In my own particular case I well remember that I early had found many incongruities and entertained many misgivings. But at a continent's breadth away no satisfactory test was possible. The phenomena were not to be viewed at a distance. I long craved to inspect conditions at close range. Opportunity at last came.

With the penchant of the army officer for dramatic effect rather than for the coolly balanced judgement of the scientist, Major Powell surely gathered about him a personnel having brilliant imaginative powers. But in initiating the rather unscientific Powellian policy of geologic *saisissement* Captain Dutton entirely missed the larger physiographic significance of his important observations on the prodigious erosion which the Utah region had manifestly undergone in relatively late geologic times. He readily fell into speculations along other lines. With him inductive reasoning took tectonic rather than physiographic turn.

Dutton's main calculation was an estimate of the crustal effects of sedimental loading and unloading through vast erosion. He endeavored to demonstrate that the converse of Hershel's thesis of crustal insinking was also true. Therefore, the Utah and Great Basin tracts, from which such enormous volumes of rock waste had been so lately removed, should display measurable evidence of continual and notable upraising. As a definite thesis the hypothesis of isostasy was just beginning to take form.

With bent of mind so decidedly tectonic Dutton turned eyes to the westward of the Utah High Plateaus for illustrative substantiation of his theory. In the Great Basin ranges he firmly believed that he should find satisfactory proofs. Like Gilbert, Powell, and Russell, he fancied the erosional derivation of these desert mountains to be entirely out of the question. By this famous quartette the tremendous potency of eolation, or wind-scour, in arid lands was little suspected.

When the arid Utah mountains first became the subject of

especial geologic description, about 1880, no such thing as a distinctive desert geology was entertained. Possibility of a definite geographic cycle in land sculpture was one of the modern earth concepts yet undreamed. Competency of the wind as a general erosive agency was not yet established. Operation of epeirogenic movements was just beginning to be distinguished and was yet but little understood. Omission of such basic considerations, as we now know them to be, necessarily led to curious aberration in interpretation of the phenomena presented by lands of little rain.

When I first took up residence on the arid Mexican tableland it was with special reference to being near the enticing novelties of desert tectonics and the possibilities of its practical relations as an aid to mining. Before entering the field I already had many grave doubts concerning the alleged genesis of its make-up. I particularly desired to lay hands, as it were, on the reputed fault-lines of the mountain blocks. I wanted to find some tectonic relationship of that impossible freak — the laccolith. I wished to discover some measure of the competency of the wind under the stimulus of aridity as a graving tool in arid land sculpture.

At first glance the fault-block structure of the desert ranges seemed all too simple — as simple, perhaps, as it did to the imaginative author of the celebrated hypothesis. I fancied that I had only to go to the foot of a mountain ridge, determine off-hand the approximate location of the bounding rupture, and then find along the fault-scarp a line of miners' cabins which would enable me to read the ore signs as from a printed page. But the mines were scattered over the mountain everywhere except along the piedmont scarp where Duttonian argument demanded. Not to be deterred by so unpropitious a beginning examination of the cross-chasms was instituted. The Tijeras Canyon, deep as the Royal Gorge, separating the lofty Sandia and Manzano sierras, gave a section a mile in vertical exposure to the very roots of the mountains. Palomas Gap, likewise deep, between the Sierras de los Caballos and San Cristobal, gave similar magnificent prospects from top of mountain to bottom. Soledad Canyon, which bisects the Sierra de los Organos, presented also gorgous views. There were innumerable other determinative exposures.

None of these numerous chasms, or cross canyons, which traversed the so-called fault-scarps showed that there had been any appreciable dislocation along the line which the scarp was supposed

to represent. The scarp proved to be in all cases the face of a recently cut rock-shelf fashioned out of solid rock much after the manner of a wave-cut cliff on an exposed coast of the sea. Nor was the scarp confined to the raised side of the tilted mountain block. In some desert ranges it marked both sides. In others all sides; in fact some were completely girdled. There was clearly no genetic relation between steep face and structure. Much prospecting had evidently been uselessly undertaken.

That among all the mountain masses of the Great Basin and the Mexican Tableland there should not appear a single instance of major faulting of recent date was surprising. Instead of being orographic blocks lately outlined, dislocated and tilted, the desert ranges proved to be mainly mountains of erosion. Longitudinal fault-lines very generally failed to characterize the piedmonts. The tectonics were manifestly chiefly relatively ancient. Wherever longitudinal faulting developed it was usually far out on the plains. Isostatic compensation surely did not obtain in the case of narrow, single mountains. Along the so-called fault-scarp ore-bodies were not to be sought. All geological conditions precluded ore localization in such situations. Small wonder, then, that mining camps did not display themselves along lines most expected.

Mine exploration has another fundamental bearing upon the verity or falsity of isostasy as usually postulated. On a scale very much larger than any desert range the cordillera of the Rocky Mountains is recently the theme of isostatic measurement by means strictly physiographic, supported by practical expression in mine development. The crustal span here is so long that if the floating-block idea be true mineralization should go on extensively at the foot of the limbs on either side. This clearly does not obtain. From earliest times this great mountain tract seems to have undergone continuous oscillation. The region is one which is repeatedly uplifted. It is one which suffers again and again tremendous planation.

Profoundest planation characterize several relatively late geologic periods. In this region especially Comanchan, Laramian, Miocene, and Recent times are conspicuously marked. Peneplanation during the first of these periods was particularly wide spread. Singularly the Jurassic ancestral Rockies do not appear to have grown higher as their summital substance wastes away, as the

Duttonian hypothesis demands. They allow themselves to be worn down to the very level of the sea without appreciable attempt at upward movement. After reaching that level when their positive volume had been completely consumed, the entire tract which lofty mountains had occupied proceeds to sink further. Downward movement continues until the old sea-level plain attains a position more than two miles beneath the surface of the epi-continental ocean. Despite the removal of its former great positive load the net result of regional vertical movement is notably negative.

For four successive geographic cycles similar procedure takes place, so that the Comanchan episode is not a solitary instance. Removal of load through erosion is not accompanied by progressive upraising. Rolling of oceanic waters over the tract follows regional unloading. Maximum sedimental loading of the area is immediately succeeded by extensive uplifting. At every stage of diastrophic activity phenomena are met that are diametrically opposed to those which Dutton postulates. Isostatic hypothesis in this instance utterly fails to sustain itself before such an array of critical testimony. Geologically the crustal span of isostatic flotation seems not to hold for cordilleran tracts the width of the state of Colorado. Orogeny manifestly rests on other telluric forces.

Decisive and convincing as are the recent geological observations bearing upon specific aspects of isostasy it is mining that gives early clue and first points out the lines of determined attack. Local geographic distribution of the mines first shows that the crustal span the breadth of the Rockies is as inadequate isostatically as is that of the narrow desert range of the Great Basin. Scarcity of mines along the lines which hypothesis, if true, demanded at once outlines the mode of solving the problem. It seems passing strange that practical and commercial data should so long antedate the strictly theoretical and scientific. We do not always realize how closely after all the theoretical and practical are intertwined. Perhaps the isostatic measurement of a compensating crustal span might have been accomplished just as well without first aid from mining. The fact remains that it did not. It may be well doubted that it ever would.

While today the general hypothesis of isostasy in the strictly Duttonian sense is steadily losing ground among geologists, it

recently receives strong support from the mathematicians. Albeit so theoretical, mathematics is a mill which grinds exceedingly fine. Yet, after all, what comes out of such mill depends very largely upon what is fed into it. So that, concerning earth problems, there is, perhaps, little to choose between rough guess of geologist and most pains-taking calculation of mathematician.

In their critical bearing upon the foundations of isostasy the clues of mining, reinforced by the recent observations on the Basin Ranges and the Rocky Cordillera, seem to require that the mathematical equations be radically modified in order properly to satisfy the necessary consequences lately derived directly from the field, or else they will have to be given up as purely fictitious.

But the mathematical hypothesis of isostasy of later years and the geological hypothesis of earlier years as upheld by Hershel, Dutton and Gilbert are in reality two very different postulates. The fallacy of Duttonian reasoning, as widely tested by practical mining operations and as carefully measured by geological calculations, quickly turns the inquiry into other and more promising directions. When the prospects of the observational geologist and the closet mathematician became hopelessly confused it was discovered that there were under consideration two entirely distinct themes. One resolved itself into simple hydrostatics, and the other into impossible tectonics. Recognizing that the isostasy of Hayforth and Bowie is not the isostasy of Dutton and Gilbert, both Becker and Davis, among others, ascribe the isostatic incongruities to tangential pressure and telluric crushing effect, which are of course not isostatic at all.

Failure of Dutton's hypothesis of isostasy to promote mine exploration is in no sense discouragement concerning the aid which scientific principles may give industrial activities. Isostasy when first formulated had no phase of ore search involved. Its extension to the latter was merely a working guess. That it did not bring desired results merely forced the problems into other and allied channels for solution. From some one of these more promising and speedy achievements must ensue.

Notwithstanding the fact that the isostatic hypothesis proves unavailing in mining its chief shortcoming points out a geological law that should be of great practical service in exploring for new mineral veins. The larger, or telluric, relations of dislocative fissure-veins seem capable of utilitarian direction. That fissure-

veins associated with profound faults have some genetic connection with telluric tectonics appears to be a necessary consequence of their orderly geologic setting. That such ore-veins are the direct, albeit somewhat different, expressions of the same compressive stresses which initiate mountain ranges and form the major plaits of the earth's crust seems to present small doubt.

The surmise is not a wholly unexpected impression derived from casual observation and fitful acquaintance with western American ore deposits. Even the remarkable uniformity of hade and the surprising sameness of trend demand for their explanation something more than mere fortuitous occurrence. What the basic law shall prove to be may not be so far away as might be imagined. When once formulated this law must constitute for modern ore exploration its most advantageous adjunct.

Notwithstanding the fact that steep dip is the most striking feature connected with Western fissure-veins, for example, the extreme uniformity usually claimed does not appear to be nearly so prevalent as might be inferred from perusal of the literature on the subject. There are many and noteworthy deviations. Despite the numerous apparent exceptions to rule and a multitude of erratic examples which have yet to be brought into accord, the frequency of small hade is doubtless to be regarded as a direct function of telluric relationship. Then, too, the variable steepness of inclination may prove eventually to be an index to geologic age — the smaller the hade the younger the fissure; and conversely, the lower the dip the greater the antiquity of the slipping. For this aspect there seems to be fundamental cause.

The larger or telluric relationships of ore-bodies never receive the genetic attention which they really merit, probably mainly for reason of their universally supposed local character. In this regard there are, indeed, few geological phenomena which are so well adapted to supply critical data as dislocative fissure-veins.

Recent laboratory experiments have a direct and curious bearing upon this very problem. All of the grander relief and structural features of our planet are so perfectly reproduced that the replicas in miniature seem to indicate that in nature they are the immediate effects of a diminishing rate of the earth's rotation upon a heterogeneous crust or zone of rock-fracture. In the fifty odd millions of years which have elapsed since Mid-Paleozoic times, when the sidereal day was only about one-fourth so long as

at present, ample opportunity is given for crustal adjustment that finds final expression in the great earth plaits and cordilleral ranges of today.

Then, also, the overthrust is generally little regarded. It is directly associated with the major flexing. No doubt it is a phenomenon that is much more widely effective than is commonly suspected. With reference to the compressive fault dislocative fissure-veins appear as subordinate expressions of the same telluric stresses. If their prevailing strike chances to be subparallel to the line of overthrust relief from strain rather than normal to it, as might be expected, it may be merely another necessary consequence of the checkered character of the rock-masses.

Although it might be inferred at first thought that fissure-veins originating under crusted stress should, in western America for example, trend north and south parallel to the strike of the thrust-rupture and also parallel to the desert range axis the fact that they do not does not necessarily indicate that their genesis is of minor instead of major nature, or that it is orogenic rather than telluric in its fundamental character.

Seemingly the secondary aspect is more apparent than real. It actually varies continually in different mountain ranges and even in the same range. This deviation appears to be partly dependable upon tortional stress according to the abruptness, or amplitude, of individual orogenic structure. The latter, of course, is entirely independent of present mountain relief, which seems to be mainly erosional in character, if latest geological advises are to be accepted.

The fact that in the majority of the desert ranges so many of the fissure-veins seem to strike nearly east and west may be due chiefly to the illusory circumstance that because of their ore-values these rupture lines are the only ones which are usually closely observed. In reality the primary jointing system of such mountain blocks is often decidedly radially disposed, the straight side of the semi-circle coinciding with the line where the steep face of the range cuts the plains surface. Any local set of fissure-veins is, therefore, only a limited and noticeable segment of a larger and more comprehensive scheme.

Besides the steep dips which are so characteristic of so many fissure-veins and the latter's prevailing easterly strike, there is a third notable feature which must be closely linked up with them.

The direction of fault movement is commonly more nearly horizontal than vertical. Contrary to usual assumption dislocative phenomena of this kind are seldom upright and gravitational. When carefully analysed the direction of the slipping is found to be very nearly on the level. Associating this feature with the high dip a warped surface is obtained the component forces of which are capable of exact mathematical expression and ready reference to the major telluric stresses imparted by the earth's rotative retardation.

Thus is emphasized the fundamental weakness of the mathematical theory of isostasy since no distinction is made between the physical behavior of the thin crustal shell of rock-fracture under telluric stress and of the thick interior mass responding to rock-flow. In view of the fact that any depression, or in sinking, of the crust due to sedimental loading occasions removal of an equal thickness from the bottom of the affected prism through direct flowage it may be questioned whether any such rock-prism appreciably changes its specific gravity, so slightly in this respect do rocks in the zone of fracture differ from one another. What really then, is mistaken for floatage phenomena is adjustment in the earth's crust of the effects of centrifugal force arising out of a retardation of rotation of the geoid.

From the miner's viewpoint it is, after all, not so strange that the Basin Ranges and the Rocky Cordillera should on the one hand so strongly belie the verity of isostasy insofar as they are directly concerned, and on the other hand should point so conclusively to the strictly telluric nature of their genesis, and indeed of orogeny in general.

Referring, then, characteristic mountain tectonics with which we are best acquainted to the secular diminution in rate of the earth's rotation all of the principal orographic features are readily and satisfactorily reproduced artificially. When, also, it is fully realized that mountain genesis appears to be merely feeble expression of the larger telluric properties of our globe we may readily adjust the dependent structures, including fissure-veins, so that search for new ore deposits shall have scientific basis, and the long sought desideratum of predictable results.

GLACIAL MAN IN AMERICA?¹

By ARTHUR M. MILLER

At the time when Columbus discovered America the descendants of the Glacial races of man already in the Western hemisphere, mainly represented by the Iroquois tribes, were all being crowded into the Atlantic ocean by the later comers from Asia. Previous to European arrival the Indians of the east coast must have in long days before these occupied the interior far beyond the Mississippi River. In the country lying to the east of the great stream their remains must be intensely sought if a complete and connected story of American occupation by man is to be spun.

As well known many of the caves and salt-licks of the Ohio Valley are celebrated for being important repositories of mammalian remains. Some of these now include the remains of prehistoric man. Since earliest occupation of the Valley by Europeans these caverns attract attention. But a hundred years ago the interest in the bones entombed in and about the licks and in the caves throughout the Ohio Valley region was much greater than in later years.

At the Big Bone Lick, Kentucky, excavating for fossil bones was a favorite pastime for such prominent personages as Gen. William Henry Harrison (1795), Doctor Goforth of Cincinnati (1804), Thomas Jefferson (1807), and John Clifford of Lexington (1816). The last important excavation was done by Prof. Nathaniel S. Shaler in 1869.

As a result of these various explorations a great quantity of bones was recovered. Teeth and bones of the Mastodon and Mammoth were especially abundant; and many found their way into the museums of this country and of Europe. There was no indication that when finally interest lagged and excavation ceased

¹ Abstract of a paper presented before the Geological Society of America, at the Amherst Meeting, December 28, 1921.

the supply of remains was by any means exhausted. Little systematic search was ever undertaken at any other salt-licks of Kentucky for these remains, though desultory digging revealed their presence in notable numbers.

Caves also came in for a large share of attention in the early days as repositories for these remains. Discovery here was mainly the result of the development of the saltpeter industry. In exploring some of these caves for the nitrous earths Dr. Samuel Brown, of Lexington, came into possession of an extinct peccary; and Mr. John Clifford, also of Lexington, obtained the skeleton of a *Megalonyx*.

About the year 1813 there was found, either in the Mammoth Cave, or one of the nearby caverns, a human mummy, which at the time excited considerable notice. Accompanied by a drawing executed by C. S. Rafinesque, it was minutely described in volume eighteen of the *Medical Repository* for the year 1814. Also, about the same time, there were obtained in the Mammoth Cave and neighboring caverns, all as the result of excavations for nitrous earths, a variety of articles of aboriginal manufacture.

During the past few years exploratory interest revived; and there was undertaken in the vicinity of Lexington the exploitation of two new caves which had been discovered a short time before. Although as yet only the surficial layers in each cave have been examined a considerable variety of bones were obtained. A newly discovered extension of the Phelp Cave yielded the remains of a bear. In another, the Smith Cave, occurred the remains of a raccoon, a ground-hog, a fox, various rodents, a large wolf, deer, a bear (believed to be the polar species), and a buffalo. Human bones also occurred plentifully.

Preliminary investigation of various other caverns and rock-shelters in different parts of the State also resulted in the finding in nearly every instance skeletal and implemental remains of man. In these caverns, rock houses and salt-licks of the lower Ohio Valley there must surely exist much interesting paleontological and archeological material awaiting only systematic exploration. Much of this material undoubtedly accumulated at that remote time when the margin of the continental ice-sheet was not so very far away. If man existed in this country during Glacial times it is here in these caverns and rock shelters and around the salt-licks that abundant evidences of him should be sought.

NEW MEXICAN LACCOLITHIC STRUCTURES

By CHARLES KEYES

Geographic Characteristics. The Sierra del Oro, or Gold Mountains, constitute one of the oldest and most famous of American mining districts. The title is that applied by the early Spanish settlers of northern New Mexico to certain conspicuous eminences rising abruptly out of the vast desert plain which encompasses the southernmost extension of the Rocky Cordillera, and which is also one of those illimitable bolsons that characterize the high Mexican tableland and the Great Basin region of western United States. Apparently these mountains rest on the bottom of a broad valley having the last ranges of the Rockies on one rim and on the other the first of the Basin ranges.

Viewed from a distance of a score of miles, from the Capitol dome at Santa Fe, through the dense gray haze of desert, the Gold Mountains appear as four groups of sharp peaks which rear their jagged heads above the surrounding plain like rocky isles above the surface of smooth summer seas; or lengthened and distorted by the desert mirage they seem through the dust fog as clusters of church spires of some great and populous city. In order from north to south the several groups of peaks are Los Cerrillos Hills, the Ortiz Mountains, the Tuertos Mountains, and the San Ysidro Mountain. Beyond are the great Sandia-Manzano ridges — the first of the so-called Basin ranges.

Besides jutting abruptly out of the plain these four mountain groups constitute curious andesitic masses embraced in a field of sediments. That the eruptive bodies are not normal extrusives is clearly indicated by the fact that the Carbonic limestones and Cretacic shales are upturned around their flanks. They are laccoliths as typical as can be found anywhere on the face of the globe.

The arrangement of these mountains along a definite line is especially noteworthy not only because of its novelty among laccoliths but because of the fact that in the underlying ancient geologic structures of the region a reason therefore is found that probably affords a satisfactory explanation for all laccolithic structures.

Since the first recognition of the laccolithic nature of Los Cerillos hills and the Ortiz Mountains little seems to have been added to our knowledge of the area except through Prof. D. W. Johnson's account.¹ Messrs. Lindgren, Gratton and Gordon² who spent considerable time in the district, appear to have entirely missed the essential structural characteristics. W. T. Lee,³ although recognizing the location of the Tijeras fault at one point near the head of the Tijeras canyon, fails to draw its proper trend, or to comprehend its great extent, stratigraphic importance, and its curious tectonic significance.

Linear Disposition of Laccolithic Sierras. All descriptions of laccolithic mountains make little or no attempt to connect the areal arrangement of the intrusions with lines of acquired geologic structures. In fact all accounts agree in assigning them a chance location. A novel feature of the New Mexican laccoliths is a recognizable regularity of their situation. The four groups there represented are arranged along a nearly straight line which runs northeast and southwest, or at an angle of about 45 degrees with the axes of both the southern Rockies and the nearest Basin ranges.

This linear disposition of the New Mexican laccoliths is hardly fortuitous. An arrangement of this description calls for some kind of tectonic dependence. Such actually seems to be the case. After proof is forthcoming in one specific instance, the query arises whether it is not so in other instances and whether it is not really a relationship that is determinable in all laccolithic masses. There is now strong presumptive evidence that this is so. One main reason why the relationship was not earlier recognized is the fact that in accordance with a bias of the hypothetical demand it was the custom to draw the ground-plan of a laccolithic group on the basis of circles having as centers the highest mountain points. Were the Sierra del Oro laccoliths projected in this way

¹ Columbia School of Mines Quart., Vol. XXIV, p. 303, 1903.

² Prof. Pap. U. S. Geol. Surv., No. 68, p. 163, 1910.

³ Bull. U. S. Geol. Surv., No. 471, p. 574, 1912.

they too would appear as erratically arranged as are the Henry Mountains of Utah, or the Judith Mountains of Montana.

Tectonically the position of these mountains so near, yet quite apart from the last vestiges of the Rocky Cordillera, is fraught with the utmost significance. To these curious relationships attention is particularly directed later. The same compressive stresses by which were corrugated the southern Rockies seem to find distinctive expression far beyond the visible geographic boundaries of the Cordillera. Analysis of the geologic structures associated with the formation of the Sierra del Oro indicate for it a composite orographic origin.

That the several laccoliths under consideration should be regularly arranged according to a definitely determined plan, instead of being promiscuously disposed, as is claimed for most mountains of this character, appears to have been predestined long prior to the date when the intrusion took place. The controlling cause is a pre-Cambrian consequence. Geological structures which were thus early initiated persist through the ages to become determining factors in the latest tectonics, and in the location of the intrusions.

Tectonic Setting of Sierra del Oro. It is extremely doubtful whether laccolithic mountains ever have the erratic disposition usually accorded them. In the case of the New Mexican examples of this class an adequate and satisfactory explanation for their regular alignment seems to be found in the older tectonics of the region. Two profound planes of fracture and displacement, parallel to each other and about four miles apart, appear to be late crustal adjustments along pre-Cambrian lines.

The trough valley, or *Graben* as the Germans term it, of which the Rhine valley is perhaps the best known example, is a structural phenomenon that is familiar enough. Its origin is readily explained by assuming that it is formed by the dropping of a faulted block. The reverse of the drop-block, or raised block, is not so well known; and its genesis is even more difficult to account for upon any known principle of mechanics. Nevertheless the phenomenon is one of not infrequent occurrence among the ranges of the Great Basin and the Mexican Tableland as well as elsewhere. On a small scale the phenomenon is commonly met with in mines where it is especially distinguished as a horse, or horst. The miner's title is applied by Suess⁴ in a larger sense to

⁴ Antlitz der Erde, I Bd., p. 167.

mountain structures. Now the New Mexican laccoliths are intimately associated with a gigantic horst, which however finds no distinctive expression in the present relief configuration of the district.

The tectonics of the great Sierra del Oro horst is well displayed at its southwestern extremity where it reaches sky at the foot of the Sandia uplift. As exposed in the deep Tijeras canyon and in Hell canyon east of Albuquerque, the essentials of the structure there revealed are best represented by cross-section diagram (figure 6). Notwithstanding the fact that its actual existence seems so well established by abundant observation among the

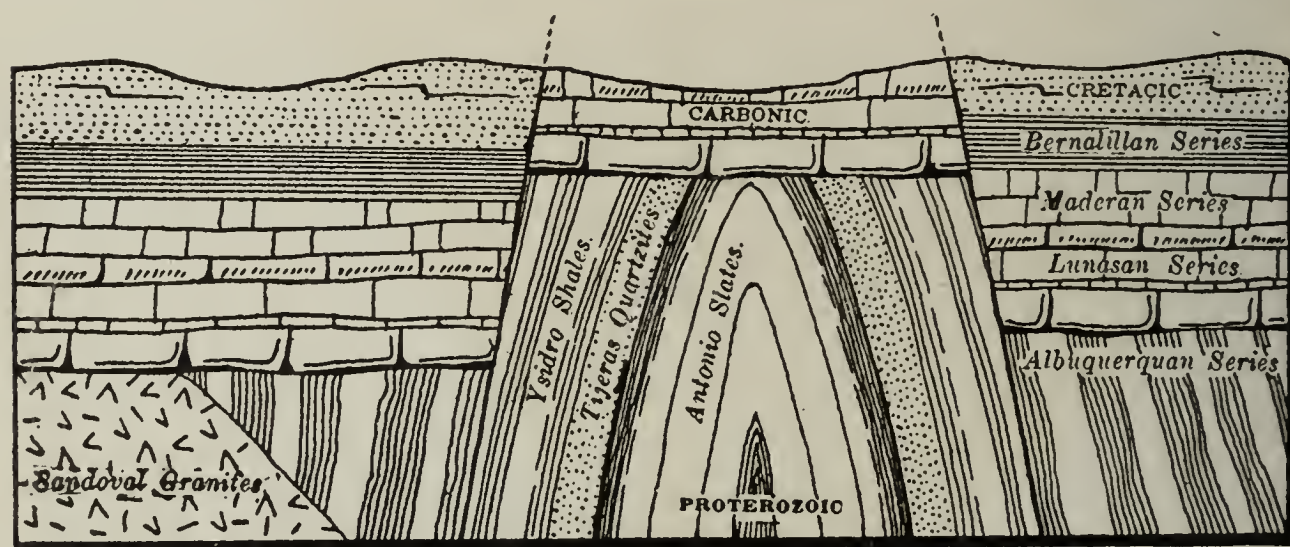


Fig. 6. Cross-section of Sierra del Oro Horst, New Mexico.

Great Basin ranges as a mechanical possibility the mountain horst remained largely a strictly hypothetical feature until the foundations of one were found exhumed in the depths of the Sandia gorges. It is manifest that the sustaining stresses inaugurated in pre-Cambrian, or early Paleozoic, times continue to remain to the present day orographic potentialities of the first order. As a tectonic curiosity, as it were the bisection of the desert range of Sandia and Manzano by the Tijeras faults was pointed out some years ago; but the full mechanical significance of the horst structure was not then fully appreciated.⁵

The chief stratigraphic horizon of the laccolithic intrusions is the great plane of unconformity at the base of the Carbonic limestones which rest directly upon the folded and beveled pre-Cambrian crystallines. The intrusives directly abut a fault-plane and the channel of magmatic supply doubtless also followed the same

⁵ Proc. Iowa Acad. Sci., Vol. XII, p. 167, 1905.

LOS CERILLOS

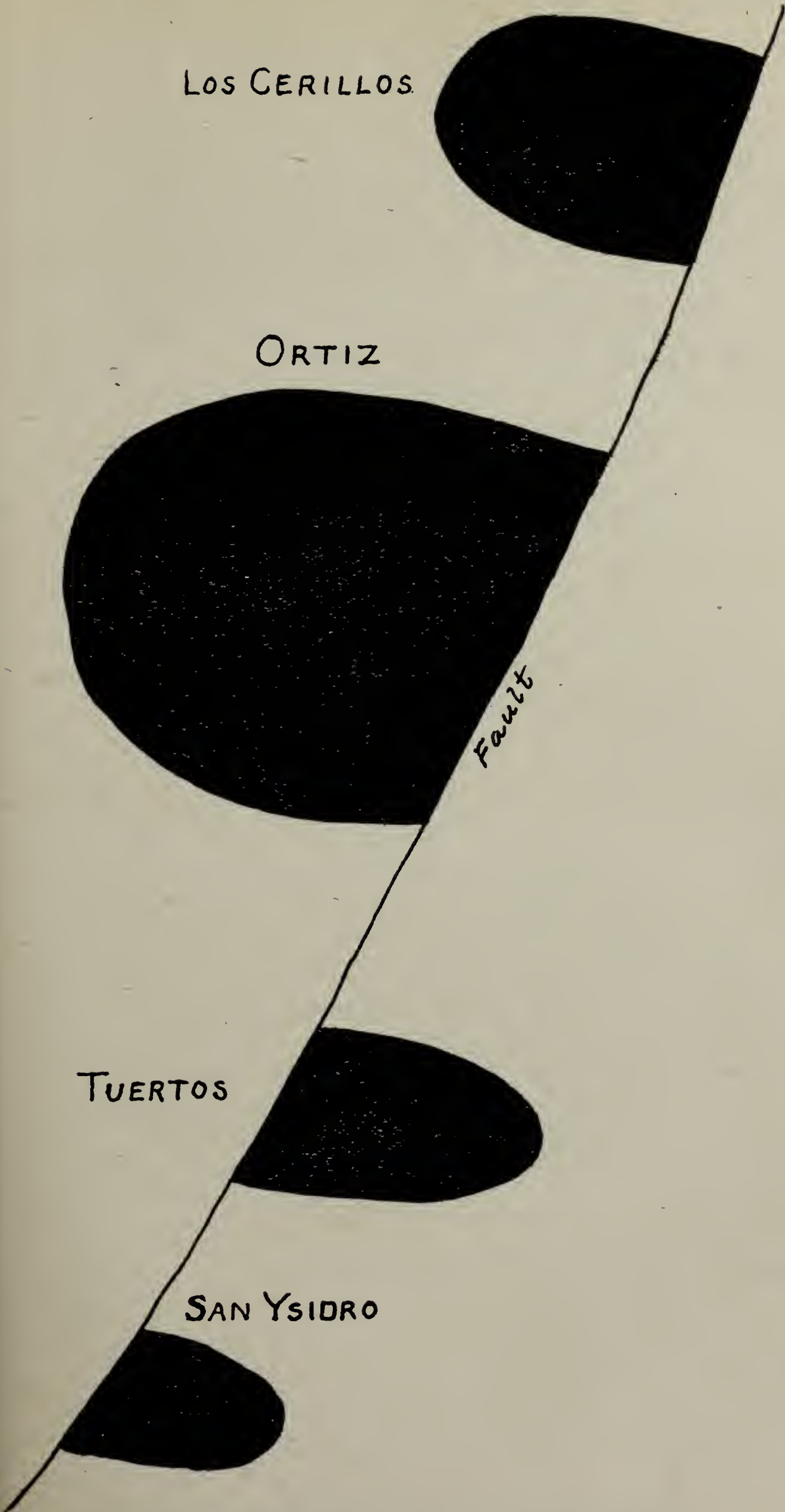
ORTIZ

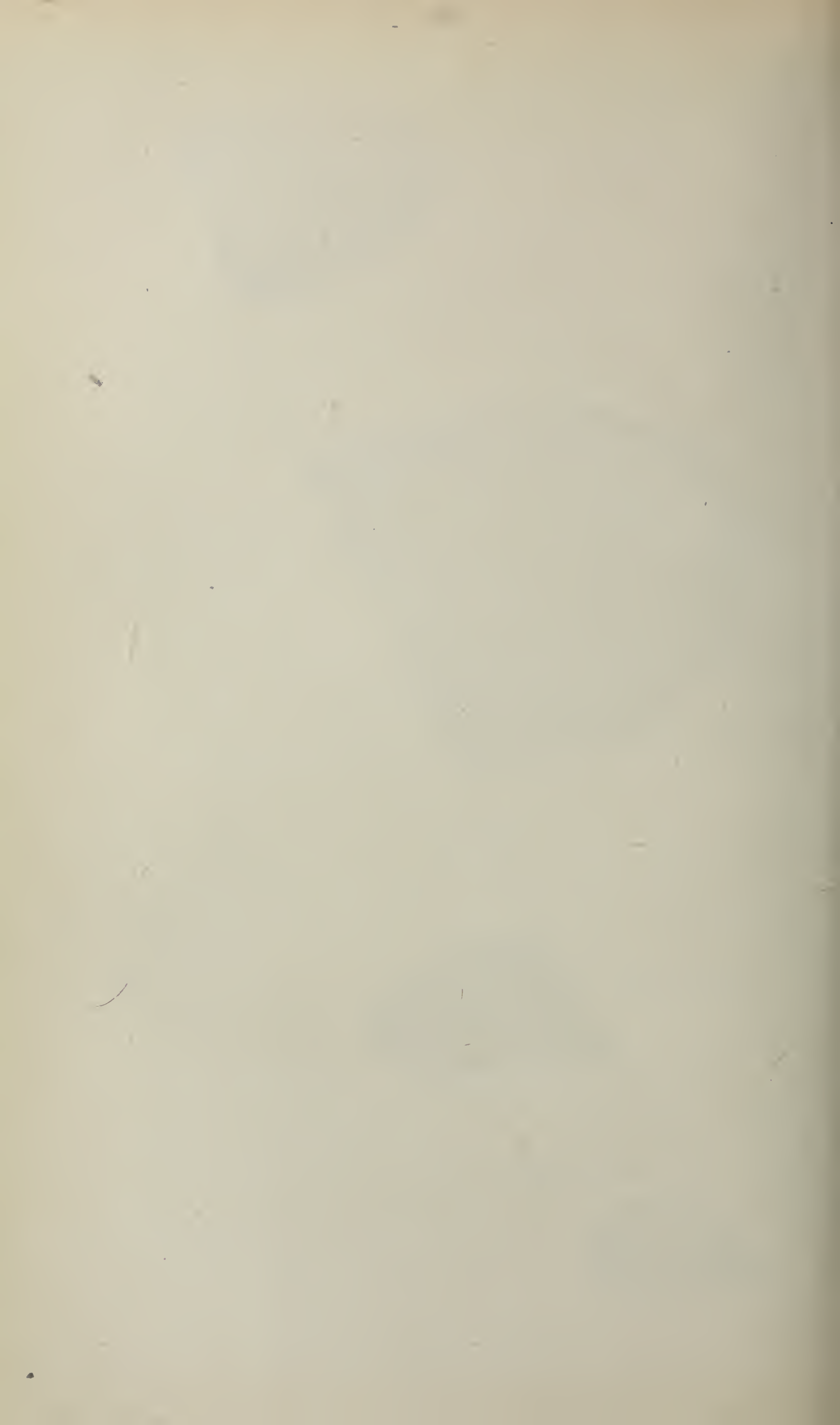
Fault

TUERTOS

SAN YSIDRO

GROUND-PLAN OF SIERRA DEL ORO LACCOLITHS





line of rupture. In this regard the intrusives are strictly interformational in nature, like those recently described by Messrs. Weed and Pirsson⁶ in the Little Rocky Mountains of Montana; and by Prof. J. A. Jagger⁷ in the Deadwood Gulch district of the Black Hills, although the first mentioned instance is more likely batholithic rather than laccolithic in character. However, the interformational nature of the laccolithic intrusions probably has no especial genetic significance, and the plane of separation of the strata is doubtless due mainly to the great ease with which parting takes place in some parts of rock-masses than in others and the resistance to ready fracture elsewhere.

Ground-plan of the Laccolithic Groups. Unlike the laccolithic members of the Henry Mountains, the Judith Mountains, or the West Elk Mountains, in which no orderly arrangement or relationship is yet recorded the cuneiform intrusive masses of New Mexico all seem to align themselves along a straight course, as shown in the annexed diagram (plate viii). In this respect the faulting appears to be an essential condition of the intrusion and in point of time preceded it. From this straight thick edge of the laccolithic wedge the intrusive mass becomes attenuated in all directions. Instead of the laccolith being lens-shaped it is really a half-lens.

In view of the circumstance that the ground-plan of laccoliths has been invariably assumed to be a circle, with a consequent erroneous perspective of position, these relationships are badly in need of strict review with conceptions in mind other than the original one. Reconstructed on a cuneiform basis instead of the blister hypothesis even the Henry Mountains appear to adjust themselves along regular lines which are parallel to the dominant tectonic features of the region — the great "Water-pocket" flexure. Deprived of their hypothetical extensions in circles around the high-peak points the Judith Mountains also assume definite tectonic relationships.

Structural Features of San Ysidro. The southernmost member of the Sierra del Oro is best known as San Ysidro, but latterly is sometimes called South Mountain. A noteworthy fact concerning this intrusive mass is that it seems to be completely surrounded on the ground surface by Cretacic sandstones and shales.

⁶ Journal of Geology, Vol. IV, p. 402, 1896.

⁷ Annals New York Acad. Sci., Vol. XII, p. 212, 1899.

It is not, however, necessarily intruded in the latter since Carbonic limestones are detected at many points on the flanks of the mountain. The laccolithic body is further distinguished by being located outside of the great horst and on the down throw side of the fault. The channel of magmatic supply no doubt lies in the fault-plane. In cross-section the structural features are indicated in the subjoined cut (figure 7).

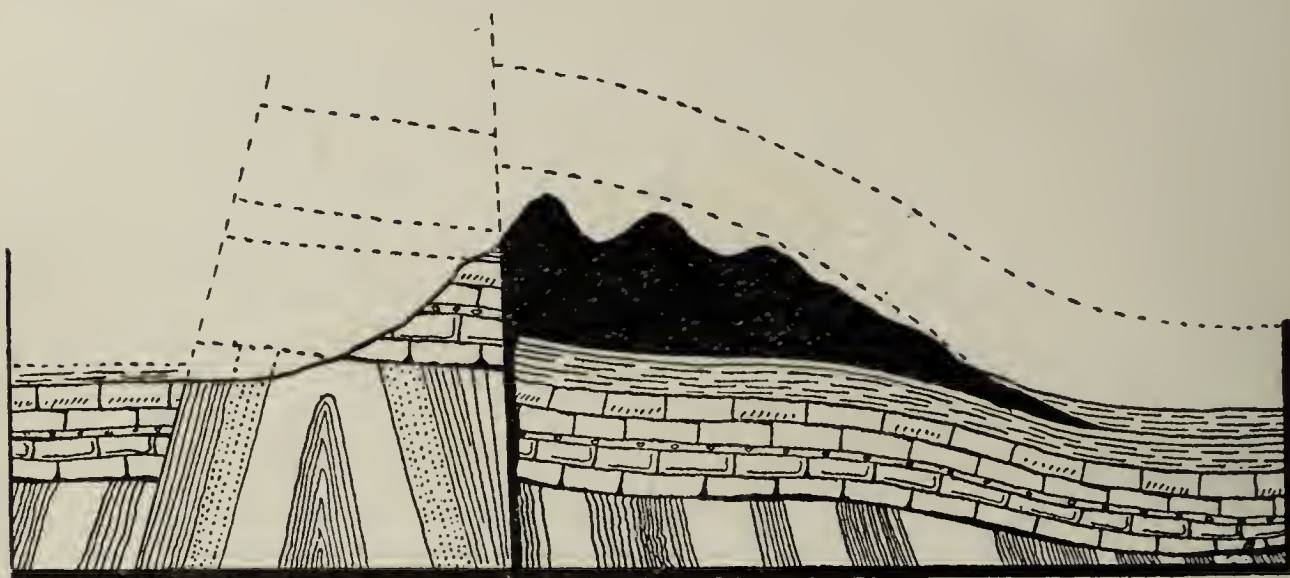


Fig. 7. Cross-section of San Ysidro Laccolith.

Compared with other laccolithic mountains which have been described a novel phase is the loosened end of the overlying prism of strata. The genetic significance of this feature in laccolith development is at once apparent. As faults go an unique situation is that there is on the same side of the plane of displacement a down-throw below and an upthrow above. However these are really widely different in date. Both of these characteristics present mechanical potentialities which are not possible with the ordinary concept of laccolith. They fully meet the objections so often urged against the blister form of intrusion.

Transverse Section of the Tuertos. In all general respects the structure of the Tuertos laccolith is identical with that of San Ysidro. The stratigraphic horizon of the intrusion is the base of the Paleozoics, which, as already mentioned, is the bottom of the Mid Carbonic limestones that rest directly upon the tilted Pre-Cambrian slates. The several relationships of the rocks are graphically represented below (figure 8).

A distinctive feature is the presence of limestone on the back of the eruptive body. These limestones are highly meta-morphosed and notable contact deposits of metallic ores are associated.

Oroche Peak, the high point of the group, lies beyond the belt of altered limerock. The cuneiform outlines of the laccolithic mass are especially conspicuous.

Bysmalithic Nature of Ortiz Mountains. The striking peculiarity of the Ortiz mass is that it occupies the full width of the

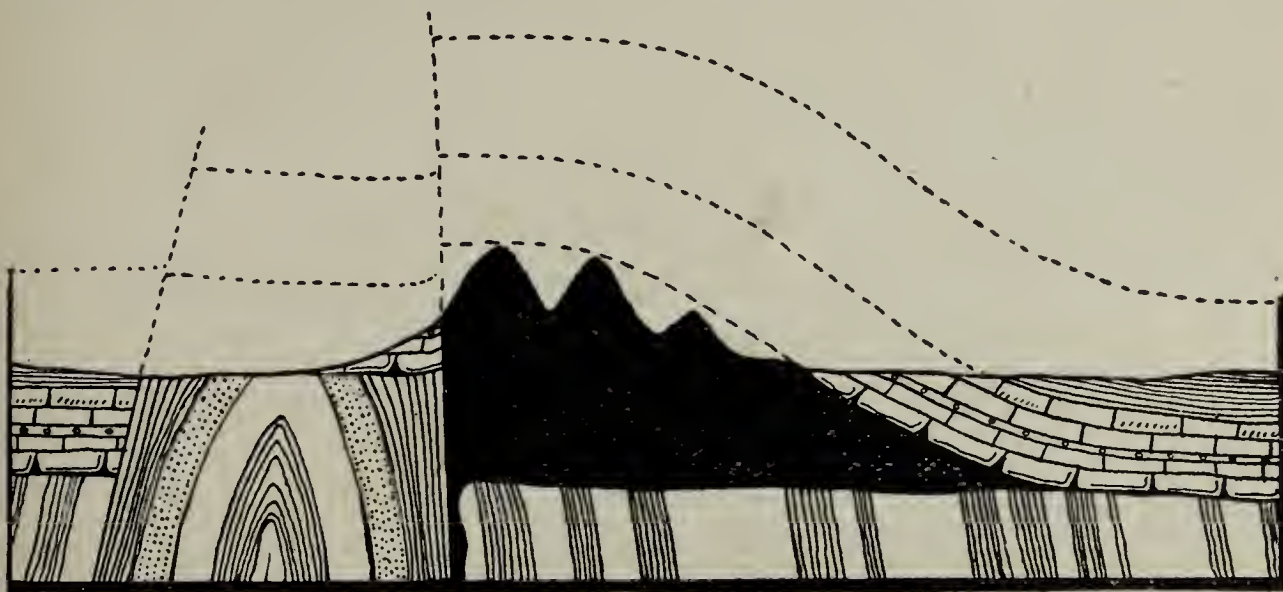


Fig. 8. Cross-section of Tuertos Laccolith.

Sierra del Oro mountain-block. It reaches from verge to verge of the great horst. Viewed in transverse section the intrusive body is in all essential respects a typical bysmalith, as so clearly differentiated from the normal laccolith by Iddings.⁸ In longitudinal outline the mass has the shape of Gilbert's ideal laccolith.⁹ The bysmalithic aspect is given in diagram below (figure 9).

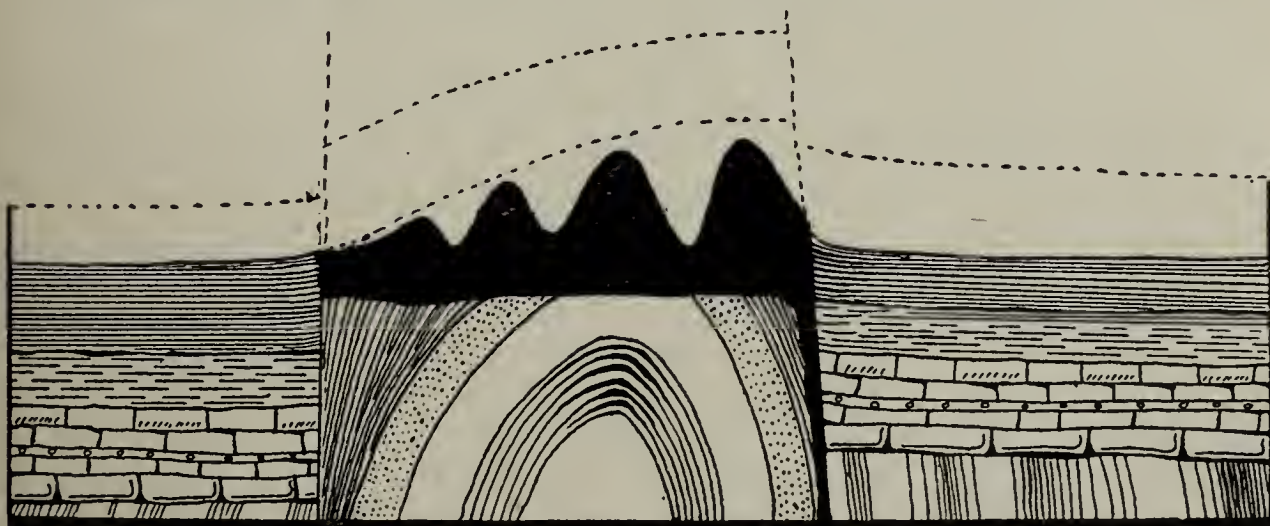


Fig. 9. Byamalithic Aspects of Ortiz Laccolith.

That faults, or lines of crustal fracture, have a controlling influence in the formation of laccoliths is nowhere better demon-

⁸ Journal of Geology, Vol. VI, p. 720, 1898.

⁹ Geology of Henry Mountains, p. 20, 1877.

strated than by the structures displayed in the Ortiz Mountains. There, also, it is completely demonstrated that a laccolith is not a thickened sheet and that the two are formed under entirely distinct physical conditions.

On the south slope of the Ortiz group the limestone cover of the intrusive body is still retained, now turned in places into garnet-rock and displaying typical contact deposits of copper. The Old Lucas mine finely shows all the phenomena peculiar to normal contact ore-bodies.¹⁰

At the eastern base of the Ortiz Mountains is the site of the old Spanish placer camp of Dolores. Running through the camp



Fig. 10. Transverse Section of Los Cerrillos Laccolith.

is a wide quartz ridge. The main interest in this ridge is that the Dolores miners believed that in it they had discovered the great "Mother Lode" —the source of all the gold of the district. On this quartz ledge a vast amount of work has been done in days gone by. When recently examined it was found to be a white quartzite instead of massive quartz, and its origin was casually ascribed to a section of one of the thick Cretacic sandstones which had been caught in the molten magma and become thoroughly metamorphosed. Later, however, an indistinguishable quartzite was noted several miles to the westward, in the Tijeras canyon, where it also had been thought to be a quartz reef, and had been extensively although vainly explored for gold. The late Prof. C. L. Herrick¹¹ thus interpreted its presence. Microscopical examination of thin slices of this rock clearly indicates the clastic origin.¹² This is the pre-Cambrian Tijeras quartzite. In all

¹⁰ Journal of Geology, Vol. XVI, p. 442, 1908.

¹¹ Bull. Univ. New Mexico, Vol. I, p. 101, 1899.

¹² Rept. Gov. New Mexico to Sec. of Interior, for 1903, p. 101, 1904.

likelihood the Dolores reef is identical, being an ancient already metamorphosed plate instead of a more modern Cretacic sandstone. This being the case it is quite possible that at Dolores it is a part of the floor of the Ortiz laccolith that is actually exposed.

Complexity of Los Cerrillos Hills. In the other groups of the Sierra del Oro only single masses constitute the laccoliths. Los Cerrillos group differs in having besides a main intrusive body numerous subordinate lenses that occupy spaces between parted strata, particularly in situations where the beds are sharply flexed. A particularly noticeable feature of the associated fault-plane is that in the same vertical line the displacement below the laccolithic body is only about 1000 feet while above the intrusive mass it is nearly 3,000 feet. A cross-section is represented annexed (figure 10).

Depth of Formation. The Sierra del Oro presents rather unique aspect for reason of the fact that it directs attention to the depths at which laccolithic intrusion takes place. This phase of the problem receives mention nowhere in connection with descriptions of this class of mountains. The discussion hinges on the nature of contact copper deposits; and the different intrusions seem to furnish the necessary evidence.

From experimental results on the conditions under which garnet-rock (the contact copper matrix) forms Chrustschoff reached the conclusion that the temperatures reaching 550 C. were indicated. High pressures were premised but no definite figures ventured. On this point Van Hise, reasoning from these physical conditions, argues for the formation of true contact ore deposits as possible in the zone of rock-flowage. This was placed at depths of from four to eight miles, where the temperatures were above that of the critical point of water, and the pressure was above 200 atmospheres.

In the Sierra del Oro we seem to have some exact figures for the thickness of the rock-pile above the contact ores, and hence for the thickness of the laccolithic cover. The laccolithic intrusions have entered at stratigraphic horizons which are definitely located in the local geologic column. The regional rock-succession is that of the west side of the Rocky Mountains rather than that of the better known east-side section. The general sequence of the rock formations and the maximum thicknesses represented in this area are as follows:

	Feet
8. Quarternaric loams and gravels.....	200
7. Tertic marls and sands.....	1500
6. Mesa Verde shales and sandstones.....	800
5. Montana shales	500
4. Colorado shales	1200
3. Mora sandstones (Cretacic).....	500
2. Carbonic limestones	2500
1. Proterozoic schists	4000

The close association of the Carbonic limestones and the Cretacic formations in this region appears due mainly to the fact that a great unconformity exists, permitting the latter often to repose directly upon the former.

The thickness of the strata overlying the uppermost of the Ortiz sills could not have been in any case, when the intrusion took place, more than over 2,000 feet; or 3,000 feet even if the intrusion is regarded as having taken place in the lower (Montana) section. Since the intrusion doubtless occurred in Early Tertic time probably not one-half of the known Tertic deposits of the region was yet laid down. A thickness of 2,000 feet of sediments would in all likelihood be nearer fact for the actual volume of strata floated upward in the formation of the Ortiz laccolith.

Tuertos laccolith, on which the San Pedro contact copper deposits are situated, is similar in all respect to the Ortiz Mountains. The geological horizon of the intrusion appears to be the same and the Montana sandstones and shales are upturned on the eastern flank of the mountains. The Carbonic limestones, on the west side in the vicinity of San Pedro, seem to be elevated and tilted on account of faulting. In any case, 3,000 feet of covering when the contact deposits were formed would be, so far as present evidence goes, a maximum. Taking into consideration the character of the rocks this figure is barely one-tenth of the thickness required to bring the horizon within the zone of rock-flowage.

MOST CRITICAL EPISODE IN EVOLUTION¹

By W. K. BROOKS

Darwin says in his *Origin of Species* "To the question why we do not find such fossiliferous deposits belonging to these assumed earliest periods prior to the Cambrian System I can give no satisfactory answer." On its geological side this difficulty is even greater than it was in Darwin's day, for we now know that the fauna of the Early Cambrian Period was rich and varied; that most of the modern types of animal life were represented in the oldest fauna which has been discovered; and that all its types have modern representatives.

Fossiliferous beds of Early Cambrian date rest upon beds which are miles in vertical thickness, and are identical in all their physical features with those which contain this fauna. They prove beyond question that the waters in which they were laid down were as fit for supporting life at the beginning as at the end of the enormous lapse of time which they represent, and that all the conditions have since been equally favorable for the preservation and discovery of fossils.

Modern discovery has brought the difficulty which Darwin points out into clearer view; but geologists are no more prepared to give satisfactory solution, although I shall now try to show that the study of living animals in their relations to the world around them does help us, and that comparative anatomy and comparative embryology and the study of the habits and affinities of organisms tell us of times more ancient than the oldest

¹ This article is the second installment of a symposium on the geological aspects of the evolution of life. It is a succinct statement of the contents of a paper which under different title originally appeared in the *Journal of Geology* for 1894, but which at that time attracted little or no attention from paleontologists. It is really the most important contribution ever made to paleontology in this country and perhaps in the world. Partly for this reason the article in condensed form is here republished. It spans a wide gap in Darwinian theory to which the author of the *Origin of Species* himself directed especial attention. Not its least important feature is its basic bearing upon the length of geologic time.—EDITOR.

fossils, and give a more perfect record of the early history of life than paleontology.

While the history of life, as told by fossils, has been slow and gradual it has not been uniform, for we have evidence of the occurrence of several periods when modification was comparatively rapid.

We are living in a period of intellectual progress; and, among terrestrial animals, cunning now counts for more than size and strength. Fossils show that while the average size of mammals has diminished since Mid Tertiary times, the size of their brains has increased more than one hundred per cent. The brain of a modern mammal is more than twice as large, as compared with its body, as the brain of its Mid Tertiary ancestors. Measured in years the Mid Tertiary Period is very remote, but is very modern compared with the whole history of the fossiliferous rocks, although more of brain development has been effected in this short time than in all preceding time from the beginning.

The later Paleozoic and early Secondary fossils mark another period of rapid change, when fitness of the land for animal life, and the presence of land plants, brought about the evolution of terrestrial animals.

I shall give reasons for seeing, in the Early Cambrian record, another period of rapid change, when a new factor, the discovery of the bottom of the ocean, began to act in the modification of species; and I shall try to show that, while animal life was abundant long before, the evolution of animals likely to be preserved as fossils took place with comparative rapidity, and that the zoological features during Early Cambrian time are of such character as to indicate that it is a decided and unmistakable approximation to the primitive fauna of the bottom, beyond which life was represented only by minute and simple surface animals not likely to be preserved as fossils.

To the zoologist nothing brings home more vividly a picture of the diversity of the Early Cambrian fauna, and of its intimate relation to the fauna of the bottom of the modern ocean, than the thought that he would have found on the old Cambrian shores the same opportunity to study the embryology and anatomy of the Pteropods and Gasteropods and Lammellibranchs, of the Crustaceans and Medusae, Echinoderms and Brachiopods that he now has at a marine laboratory; that his studies would have

followed the same lines then that they do now ; and that most of the record of the past which they make known to him would have been ancient history then. Most of the great types of ancient life show by their embryology that they run back to simple and minute ancestors which lived at the surface of the ocean, and that the common meeting point must be projected back to a still more remote time, before these ancestors had become differentiated from one another.

After we have traced each great line of modern animals as far backwards as we can through the study of fossils, we still find these lines distinctly laid down. The Early Cambric Crustacea, for example, are as distinct from the Early Cambric Echinoderms or Pteropods or Lammellibranchs as they are from these of the present day, but zoology gives us evidence that the early steps in the establishment of these great lines were taken under conditions which were essentially different from those which have prevailed, without essential change from the time of the oldest fossils to the present day, and that most of the great lines of descent were represented in the remote past by ancestors which, living a different sort of life, differed essentially, in structure as well in habits, from representatives of the same types which are known to us as fossils.

In the Echinoderms we have a well defined type represented by abundant fossils, very rich in living forms, very diversified in its modification, and therefore well fitted for use as illustration. This great stem contains many classes and orders, all constructed on the same plan, which is sharply isolated and quite unlike the plan of structure in any other group of animals. All through the sequence of fossiliferous rocks Echinoderms are found, and their plan of structure is always the same. Paleontology gives us most valuable evidence regarding the course of evolution within the limits of a class, as in the Crinoids, or the Echinoids ; but we appeal to it in vain for light upon the organization of the primitive Echinoderm, or for connecting links between the classes. To our questions on these subjects, and on the relation of the Echinoderms to other animals, Paleontology is silent, and throws them back upon us as unsolved riddles.

The zoologist unhesitatingly projects his imagination, held in check only by the laws of scientific thought, into the dark period before the times of the oldest fossils, and he feels absolutely cer-

tain of the past existence of a stem, from which the classes of Echinoderms have inherited the fundamental plan of their structure. He affirms with equal confidence that the structural changes which have separated this ancient type from the classes which we know as fossils, are very much more profound and extensive than all the changes which each class has undergone from the earliest Paleozoic times to the present day.

The zoologist does not check the flight of his scientific imagination here, however, for he trusts implicitly to the embryological evidence which teaches him that still farther back in the past, all Echinoderms were represented by a minute floating animal which was not an Echinoderm at all in any sense except the ancestral one, although it was distinguished by features which natural selection has converted, under the influence of modern conditions, into the structure of Echinoderms. He finds in the embryology of the modern Echinoderms phenomena which can bear no interpretation but this, and he unhesitatingly assumes that they are an inheritance which has been handed down from generation to generation through all the ages from the prehistoric times of zoology.

Other groups tell the same story with equal clearness. A *Lingula* is still living in the sand-bars and mud-flats of the Chesapeake Bay, under conditions which have not effected any essential change in its structure since Cambrian time. Who can look at a living *Lingula* without being overwhelmed by the effort to grasp its immeasurable antiquity; by the thought that while it has passed through all the chances and changes of geological history, the structure which fitted it for life on the earliest Paleozoic bottom is still adapted for a life on the sands of the modern sea floor?

The everlasting hills are the type of venerable antiquity; but the *Lingula* has seen the continents grow up, and has maintained its integrity unmoved by the convulsions which have given the crust of the earth its present form. As measured by the time-standards of the zoologist *Lingula* itself is modern, for its life history still holds locked up in its embryology the record, repeated in the development of each individual, of a structure and a habit of life which were lost in the unknown past at the time of the Early Cambrian period, and it tells us vaguely but unmistakably of life at the surface of the primitive ocean, at a time

when it was represented by minute and simple floating ancestors.

Broadly stated, the history of each great line has been like that of the Echinoderms and the Brachiopods. The oldest Pteropod or Lamellibranch or Echinoderm or Crustacean or Vertebrate which we know from fossils exhibits its own type structure with perfect distinctness, and later influences have done no more than to expand and diversify the type, while anatomy fails to guide us back to the point where these various lines met one another in a common source, although it forces us to believe that the common source once had an individual existence. Embryology teaches that each line once had its own representative at the surface of the ocean, and that the early stages in its evolution have passed away and left no record in the rocks.

Modern microscopic research has shown that the simple unicellular plants, and the Globigerinæ and Radiolarians which feed upon them, are so abundant and prolific that they meet all demands and supply the food of all the animals of the ocean. This is the fundamental conception of marine biology. The basis of all life in the modern ocean is found in the micro-organisms of the surface. This is not all. The simplicity and abundance of the microscopic forms and their importance in the economy of nature show that the organic world has gradually taken shape around them as a center, or starting point, and has been controlled by them. They are not only the fundamental food-supply, but the primeval supply, which has determined the whole course of the evolution of marine life. Pelagic plant-life of the ocean has retained its primitive simplicity on account of the very favorable character of its environment, and the higher rank of littoral vegetation and that of the land is the result of hardship.

On land the mineral elements of plant-food are slowly supplied, as the rains dissolve them; limited space brings crowding and competition for this scanty supply; growth is arrested for a great part of each year by drought or cold; the diversity of the earth's surface demands diversity of structure and habit, and the great size and complicated structure of territorial plants are adaptations to these conditions of hardship.

At the surface of the ocean the abundance and uniform distribution of mineral food in solution; the area which is available for plants; the volume of sunlight and the uniformity of temperature are all favorable to the growth of plants; and as each plant

is bathed on all sides by nutritive fluid, it is advantageous for the new plant-cells, which are formed by cell-multiplication, to separate from one another as soon as possible, in order to expose the whole of their surface to the water. Cell-aggregation, the first step toward higher organization, is therefore disadvantageous to the pelagic plants, and as the environment at the surface of the ocean is so monotonous, there is little opportunity for an aggregation of cells to gain any compensating advantage by seizing upon a more favorable habitat. The pelagic plants have retained their primitive simplicity; and the most distinctive peculiarity of the microscopic food-supply of the ocean is the very small number of forms which make up the enormous mass of individuals.

All the animals of the ocean are dependent upon this supply of microscopic food, and many of them are adapted for preying upon it directly, but a review of the animal kingdom will show that no highly organized animal has ever been evolved at the surface of the ocean although all depend upon the food-supply of the surface.

Animals which now find their home in the open waters of the ocean are, almost without exception, descendants of forms which once lived upon or near the bottom, or along the sea-shore, or upon the land; and all the exceptions are simple animals of minute size. A review of the whole animal kingdom would take more space than we could spare, but it would show that the evidence from embryology, from comparative anatomy, and from paleontology, all bears in the same direction and proves that every large and highly organized animal in the open ocean is descended from ancestors whose home was not open water but solid ground, either on the bottom or on the shore.

Embryology also gives us grounds for believing that all these animals are still more remotely descended from minute and simple pelagic ancestors, and that the history of all the highly organized inhabitants of the water has followed a roundabout path from the surface to the bottom and then back into the water. When this fact is seen in all of its bearings and its full significance is grasped, it is certainly one of the most notable and instructive features of evolution.

In view of these facts we cannot but be profoundly impressed by the thought that all the highly organized marine animals are products of the bottom or the shore or the land, and that while

the largest animals on earth are pelagic the few which are primitively pelagic are small and simple. The reason is obvious. The conditions of life at the surface are so easy that there is little fierce competition, and the inorganic environment is so simple that there is little chance for diversity of habits.

Growth of terrestrial plants is limited by the scarcity of food, but there is no such limit to the growth of pelagic plants or animals which feed upon them; and while the balance of life is no doubt adjusted by competition for food this is never very fierce, even at the present day, when the ocean swarms with highly organized wanderers from the bottom and the shore. Even now the destruction or escape of a microscopic pelagic organism depends upon the accidental proximity or remoteness of an enemy rather than upon the defense or protection, and survival is determined by space relations rather than a struggle for existence.

Easy character of pelagic life is shown by the fact that the larvæ of innumerable animals from the bottom and the shore have retained the pelagic habit; and I shall soon give reasons for believing that the larva of a shore animal is safer at sea than near the land.

It is not probable that bottom life was first established in shallow water, or before the physical conditions had become favorable at considerable depths. The sediment near the shore is destructive to most surface animals, and recent explorations have shown that a stratum of water of very great thickness is necessary for the complete development of the floating microscopic fauna and flora, and it is a mistake to picture them as confined to a thin surface layer. Pelagic plants probably flourished as far down as light penetrates; and pelagic animals are abundant at very great depths. As the earliest bottom animals must have depended directly upon the floating organisms for food, it is not probable that they first established themselves in shallow water, where the food supply is both scanty and mixed with sediment; nor is it probable that their establishment was delayed until the great depths had become favorable to life.

Belts around elevated areas far enough from the shore to be free from sediment, and deep enough to permit the pelagic fauna to reach its full development above them, are most favorable spots, and paleontological evidence shows that they were seized upon very early in the history of the life on the bottom.

It is probable that colony after colony was established on the bottom and afterwards swept away by geological change like a cloud before the wind, and that the bottom fauna which we know was not the first. Colonies which started in shallow water were exposed to accidents from which those in great depths were free. In view of our knowledge of the permanency of the sea-floor and of the broad outlines of the continents, it is not impossible that the first fauna which became established in the deep zone around the continents may have persisted and given rise to modern animals. However this may be, we must regard this deep zone as the birth-place of the fauna which has survived; as the ancestral home of all the improved metazoa.

The effect of life upon the bottom is more interesting than the place where it began, and we are now to consider its influence upon animals, all of whose ancestors and competitors and enemies had been previously pelagic. The cold, dark, silent, quiet depths of the sea are monotonous compared with the land, but they introduced many new factors into the course of organic evolution.

It is doubtful whether the animals which first settled on the bottom secured any more food than floating ones, but they undoubtedly obtained it with less effort, and were able to devote their superfluous energy to growth and to multiplication, and thus become larger and increased in numbers faster than pelagic animals. Their sedentary life must have been favorable to both sexual and asexual multiplication; and the tendency to increase by budding must have been quickly rendered more active; and one of the first results of life on the bottom must have been to promote the tendency to form connected cormi, and to retain the connection between parent and the bud until the latter was able to obtain its own food and to care for itself. The animals which first acquired the habit of resting on the bottom soon began to multiply faster than their swimming allies, and their asexually produced progeny, remaining for a longer time attached to and nourished by the parent stock, were much more favorably placed for rapid growth. As the animals on the bottom live on the surface, or at least in a thin stratum, while swimming animals are distributed through solid space, the rapid multiplication of bottom animals must soon have led to crowding and to competition, and it quickly became harder and harder for new forms from the open water

to force themselves in among the old ones and colonization soon came to an end.

Nothing could illustrate the fierceness of the struggle for food among the animals on a crowded sea-bottom more vividly than the emptiness of the water in coral sounds where the bottom is practically one enormous mouth. The only larvæ which have much chance to establish themselves for life are those which are so fortunate as to be swept out into the open ocean where they can complete their larval life under the milder competition of the pelagic fauna, and while it is usually stated that the larvæ of bottom animals have retained the pelagic habit for the purpose of distributing the species it is more probable that it has been retained on account of its comparative safety.

These facts show that competition must have come quickly after the establishment of the first fauna on the bottom, and that it soon became very rigorous and led to severe selection and rapid modification; and we also remember that life on the bottom brought with it many new opportunities for divergent specilization and improvement. The increase in size which came with economy of energy increased the possibilities of variation and led to the natural selection of peculiarities which proved the efficacy of the various parts of the body in their functions of relation to one another, and this has been an important factor in the evolution of complicated organisms.

The new mode of life also permitted the acquisition of protective shells, hard-supporting skeletons, and other imperishable parts, and it is therefore probable that the history of evolution in later times gives no index as to the period which was required to evolve from small, simple pelagic ancestors the oldest animals which were likely to be preserved as fossils. Life on the bottom also introduced another important evolutionary influence — competition between blood-relations. In those animals which we know most intimately divergent modification, with the extinction of connecting forms, results from the fact that the fiercest competitors of each animal are its closest allies, which, having the same habits, living upon the same food, and avoiding the enemies in the same way, are constantly striving to hold exclusive possession of all that is essential to their welfare.

When a stock gives rise to two divergent branches each escapes competition with the other so far as they differ in structure or

habits, while the parent stem, competing with both at a disadvantage, is exterminated. Among the animals which we know best evolution leads to a branching tree-like genealogy with the topmost twigs represented by living animals while the rest of the tree is buried in the dead past.

Even at the present day things are somewhat different in the open ocean; and they must have been very different in the primitive ocean, for a pelagic animal has no fixed home, one locality is like another, and competitors and enemies of each individual are determined in great part by accidents. We accordingly find, even now, that the evolution of the pelagic animals is often linear instead of divergent. Ancient forms, such as sharks, often live on side by side with the later and more evolved forms.

Many of the oldest fossils, like the Pteropods, are modified descendants of ancestors with hard parts, and there is no reason to suppose that the first animals which were capable of preservation as fossils have been discovered; but it is interesting to note that the oldest known fauna is an unmistakable approximation to the primitive fauna of the bottom.

The Cambrian fauna is usually regarded as a half-way station in a series of animal forms which stretches backwards into the past for an immeasurable period; and it is even stated that the history of life before Cambrian times is larger by many fold than its history since. So far as this opinion rests on the diversity of types in Cambrian times it has no good basis; for if the views here advocated are correct, the evolution of the ancestral stems took place at the surface and all the conditions necessary for the rapid production of types were present when the bottom fauna first became established.

As we pass backwards towards Early Cambrian times we find closer and closer agreement with the zoological conception of the character of primitive life on the bottom. Although we cannot regard the oldest fossil fauna which has been discovered as the first which existed on the bottom, we may feel confident that the first fauna of the bottom resembled that of the Early Cambrian rocks in its physical conditions and in its most distinctive peculiarities. We must regard it as a decided and unmistakable approximation to the beginning of the modern fauna of the earth, as distinguished from the more ancient and simple fauna of the open ocean.

SERIAL AFFINITIES OF SILURIC FORMATIONS IN
NORTHEASTERN MISSOURI

BY CHARLES KEYES AND R. R. ROWLEY

In several respects the Siluric succession in northeastern Missouri is one of the most instructive geological sections within the limits of that state. Owing to unusual diastrophic conditions this section occurs in a part of the Mississippi valley where it is wholly unexpected. Although so meagerly displayed as to be all comprised within the space of scarcely a score of feet it represents by deposition and by interval more than two-thirds of the entire Siluric period. It is divided medially by two marked planes of unconformity. There is, as we know, an overlapping of a southern, earlier Siluric deposition by northern, later Siluric formations. In their bearing upon the general stratigraphy of the state some of these facts have a far-reaching significance.

In the limited section appear to be represented three great rock-series, here so attenuated that they have not only been long neglected, but their essential features have been wholly misinterpreted. The relationships of the several beds recently disclosed by the sections furnish for the first time clue to the solution of a number of important stratigraphical problems. The testimony of the fossils is in strict accordance with facts established by a critical consideration of the stratigraphy.

Furthermore, no more illuminating example exists emphasizing the urgent need of directly applying the broader paleogeographical principles to the solution of local problems in stratigraphy than that presented by the recent attempt to delimit sundry taxonomic groups of terranes in this part of the continental interior.

In northeastern Missouri the older Paleozoic rocks are brought to the surface through means of profound displacement to the south of the chief Siluric outcrops. Stratigraphically the cities of St. Louis and Keokuk are on the same level. At both points the uppermost parts of the Paleozoic section are the surface

rocks. The last mentioned place is in the bottom of the great geotectonic structure known as the Keokuk trough. From this location along the line of the Mississippi river the strata rapidly rise until in the vicinity of Winfield, in Lincoln county, Missouri, rocks of the very base of the Paleozoic succession came to sky. At Louisana, in Pike county, Siluric and Ordovician beds rise above the river-level.

The uprising of this great crustal block is intimately associated with a profound dislocation, generally termed the Cap-au-Grès fault, the line of which crosses the Mississippi river a few miles above the mouth of the Illinois river. This fault is the longest, most profound and most important break in strata in all the Mississippi valley.

Several years ago this great fault-line was traced¹ from the point where it shows best near the mouth of the Illinois river northwestward through Lincoln and Pike counties.

That part of the general terranean succession which is especially instructive in the present connection comprises numbers 2, 3 and 4. The lower beds are particularly well displayed in the deep valley of Noix creek; the upper beds are exposed only in the southern part of Pike county and in Lincoln county.

Siluric Section in Northeast Missouri

	FEET
8. Louisana limestone	50
7. Saverton shales (green)	40
6. Grassy shales (black) (Carbonic)	60
<i>Unconformity</i>	
5. Limestone, reddish, heavily bedded (Devonic)	10
<i>Unconformity</i>	
4. Sexton limestone, buff, (Siluric)	30
<i>Unconformity</i>	
3. Bowling Green dolomite, brown, (Siluric)	20
<i>Unconformity</i>	
2. Noix limestone, yellow, locally oolitic (Siluric)	10
<i>Unconformity</i>	
1. Shales, blue (Ordovician)	60

The title Noix, when first proposed as a distinct formational

¹ Proc. Iowa Acad. Sci., Vol. V, p. 58, 1898.

name, referred to the white, massive oolite,² well exposed in many places in the eastern parts of Pike county. At the same time it was stated that the terrane appeared to be represented elsewhere in the county by a yellow limestone that was not oolitic in its lithologic character. Afterwards E. O. Ulrich³ extended the term to cover its normal equivalent rather than the local oolitic phase alone, to which T. E. Savage⁴ had given the name Cyrene member.

In Pike county the white oolite in one or two massive layers has a thickness of four to ten feet. It contrasts strongly with the soft blue Ordovician shales beneath; and with the equally striking brown, earthy dolomites above.

The oolitic phase of the terrane occupies an area of about 100 square miles, chiefly in Pike county, between the line of the St. Louis and Hannibal railroad and the Mississippi river. The formation is best exposed in the valley of Noix creek west of Louisiana, and along the bluffs of the Mississippi river. Outcrops occur in northeastern Lincoln county where they occupy small isolated areas in the tops of the hills.

On the east side of the Mississippi river this oolite is well displayed in the cliffs bordering the great stream. It is especially well shown near Hamburg, in Calhoun county, Illinois.

West and south of the line of the St. Louis and Hannibal railroad the oolite is replaced by normal yellow limestone.

The Bowling Green dolomite is a massive, brown, earthy magnesian limestone. It is well displayed to a thickness of 30 feet around the town of Bowling Green, in Pike county, Missouri. Farther to the east, near the Mississippi river, it is only four to five feet thick. Southward in Calhoun county, Illinois, it also attains a thickness of more than twenty-five feet. Northward, it increases rapidly in vertical measurement, as is indicated in numerous deep-well records.

As displayed in Pike county the Bowling Green dolomite appears to contain a very few organic remains. According to Savage⁵ it constitutes the upper portion of a formation which is very much thicker to the southward, in southeast Missouri, to which the name Edgewood formation is applied. On the other

² Proc. Iowa Acad. Sci., Vol. V, p. 62, 1898.

³ Bull. Geol. Soc. America, Vol. XXII, p. 608, pl. xxxiii, 1911.

⁴ Amer. Jour. Sci., (4), Vol. XXVIII, p. 509, 1909.

⁵ Illinois Geol. Surv., Bull. 23, p. 19, 1913.

hand it is considered as being the attenuated southern extension of an important rock-series of Iowa and the upper Mississippi valley.⁶

The Bowling Green formation is not widely understood; and its features are often misinterpreted. The lower part of the buff-brown limestones, or dolomites, are commonly regarded as belonging to the typical Bowling Green section. This is by no means the case. The type-section near the town of Bowling Green displays at the base about a foot of yellow fossiliferous beds which ordinarily appear as part of the main body of dolomite. When the geographic title was first proposed for the formation these bottom layers were not considered.⁷ A lower buff limestone was noted at Cyrene, and other places south of the original locality, and it was regarded as the equivalent of the Noix oolite. To these basal beds Savage⁸ afterwards gave the name Cyrene member.

In many of the Bowling Green sections in southern Pike county and in the northern portion of Lincoln county the Sexton member is commonly overlooked; and the entire section called the Bowling Green member.

Sexton limestone, as it occurs in Pike County, Missouri, consists of only a few feet of massive, gray beds which were recently designated by Savage⁹ the Sexton formation. Farther to the south in Lincoln county, this formation, with a thickness of nearly twenty feet, is recognized, although not by name, by W. B. Potter.¹⁰ Number 4 of the section given, appears to be the Sexton limestone, assigned a Devonian age. The basal layers constitute the *Pentamerella* zone.

The Sexton limestone is the upper body of the massive formation called by A. H. Worthen¹¹ the Niagara limestone, as exposed on the Mississippi river, in Calhoun county, Illinois. Since it there attains a thickness of more than 50 feet it is probable that its vertical measurement in Lincoln county, Missouri, is very much greater than has been heretofore reported.

Marking the base of the Sexton limestone appears to be a well-

⁶ Missouri Geol. Surv., Vol. IV, p. 30, 1894; also, Am. Jour. Sci., (4), Vol. XXXVII, p. 254, 1914.

⁷ Proc. Iowa Acad. Sci., Vol. V, p. 60, 1898.

⁸ Am. Jour. Sci., (4), Vol. XXVIII, p. 509, 1909.

⁹ Am. Jour. Sci., (4), Vol. XXVIII, p. 509, 1909.

¹⁰ Missouri Geol. Surv., Iron Ores and Coals, Pt. ii, p. 244, 1873.

¹¹ Illinois Geol. Surv., Vol. IV, p. 6, 1870.

defined plane of unconformity. This fact may indicate that the terrane is the correlative of the Goweran series of the region to the north, best displayed in northeastern Iowa.¹²

The nether horizon of the Siluric succession is sharply defined. Everywhere throughout the area under consideration in which they are exposed the Siluric limestones are underlain by blue shales of Ordovician age. In Pike county these shales attain a thickness of more than seventy feet. They doubtless belong to that part of the general geological section which, in Iowa, is known as the Maquoketan series,¹³ although in northeast Missouri the special title Buffalo shales is sometimes applied to them.¹⁴ In southern Missouri and southern Illinois these shales are known as the Thebes formation. In the east, in Indiana and Ohio, they pass under the title of Richmond, or Cincinnati, shales.

South of the Missouri river there comes in between the Noix limestone or basal terrane of the Siluric of the northeastern part of the state, and the Maquoketan shales, other formations which are wholly unrepresented by formations in the north. Most important of these is the Girardeau limestone.

The Siluric beds thus rest in marked unconformity upon the Ordovician strata. This fact is a significant one in all stratigraphic considerations of the Siluric section of the region. It points to far-reaching diastrophic movements which ushered in Siluric deposition in the area and to profound changes in the local sedimentation in the region.

Whether represented by the conspicuous white oolite, with its prolific fauna, or by the soft, yellow limestone, which is non-oolitic and highly fossiliferous, the Siluric succession of rocks in northeast Missouri is everywhere at the base sharply set off from all rocks beneath.

The delimitation of Siluric terranes is also sharply marked. At Louisiana there immediately overlies the Bowling Green dolomite a black shale having a thickness of about three feet. Above the black layer are a few feet of green shale; then follows the thick Louisiana limestone. West of the city five or six miles, on Grassy creek, the black shales attain a thickness of 40 feet; and

¹² Iowa Geol. Surv., Vol. XXII, p. 155, 1913.

¹³ Iowa Geol. Surv., Vol. XXII, p. 155, 1913.

¹⁴ Proc. Iowa Acad. Sci., Vol. V, p. 61, 1898.

the green shales an even greater thickness. Further to the north blue shales immediately underlie the black shales.

From the facts stated it is quite clear that the Siluric strata are seemingly continuous above with the Devonian formations of the region; but that the black Grassy shales rest in marked unconformity upon the bevelled edges of all older rocks, in places lying directly upon the Bowling Green limestone.

The nature of lower medial unconformity is interesting. As if it were not enough to have the Siluric section of northeast Missouri delimited above and below by notable planes of unconformity there appear to be two even more remarkable stratigraphic breaks running directly through the middle of the sequence. When in an earlier reference¹⁴ to the Siluric rocks of the region correlation was made with the Niagaran series of Iowa the basis was mainly that of lithologic similarity and stratigraphic succession, because fossils were not then obtained. The lower unconformity was recognized at the time but not its full significance.

The real significance of the lower medial unconformity appears now to be that of an overlap of formations due to appreciable diastrophic movements in the region at the time of deposition.

The feature of upper medial unconformity are also instructive. The evidences of unconformity between the Sexton limestone and the rest of the Siluric beneath are not at once apparent. The magnitude of the stratigraphic break is made more appreciable through generalization. The Sexton limestone does not appear to be represented anywhere in the northern part of Pike county, Missouri. It is possible that it really does extend into the northern part of the county and that some of its lower layers have been mistaken for portions of the Bowling Green member.

The particular type of this unconformity is one of marked discordance in sedimentation that is produced by overlap of formations or of marine transgression. The stratigraphic significance of this feature is important. Relationships of the contiguous formations are such that for the geological age of the Sexton terrane there would be suggested correlation with the Late, or at least Mid, Siluric deposition of the north, rather than with Early Siluric rocks of the south, as regarded recently by Savage. Paleogeographical considerations indicate for the Sexton lime-

¹⁴ Missouri Geol. Surv., Vol. IV, p. 30, 1894.

stone Late Siluric age, as is well shown by some of the more recent paleogeographic maps of North American continent by Schuchert¹⁵ and Willis.¹⁶

In considering the correlation of the several formations the genetic affinities of lower part of section is most important. That portion of the Siluric section beneath the lower medial plane of unconformity — the oolite and its equivalent, the yellow limestone, both highly fossiliferous — appears to become rapidly thicker towards the south. New beds come in below. In southern Missouri a great series is formed the main member of which is a terrane widely known as the Girardeau limestone.

According to Savage¹⁷ this southern sequence constitutes the main part of the Alexandrian series and is regarded as Early Siluric in age. By slight emendation of the original proposal so as exclude all the Siluric above the lower medial unconformity the term Alexandrian Series becomes a fitting and valid title for the rocks under consideration.

The stratigraphic equivalency of middle part of section is no longer uncertain. Above the lower medial plane of unconformity the Siluric sequence is heavy, buff, or brown dolomite. It carries only a few fossils. The formation becomes thicker to the north of Pike county. Through means of deep-well records it is easily traceable into northern Iowa. This part of the section is therefore paralleled with Mid Siluric succession of that state.¹⁸ There the Niagaran series comprises four distinct dolomites — the Sabula, Colesburg, Hartwick, and Monticello formations.¹⁹

The Bowling Green dolomite apparently has no recognizable equivalent in south Missouri. The organic remains which are found in it are few in number as well as species, and are such forms which have extended vertical range. For this reason the fossils are by no means decisive factors in the detailed parallelism of the beds. Other correlative criteria have to be mainly relied upon.

The correlation of upper part of section brings in another problem. The Siluric section above the upper medial unconformity seems illy correlated with the succession of the Early part

¹⁵ Bull. Geol. Soc. America, Vol. XX, p. 533, 1910.

¹⁶ U. S. Geol. Surv., Prof. Pap. 71, p. 226, 1912.

¹⁷ Am. Jour. Sci., (4), Vol. XXV, p. 434, 1908.

¹⁸ Am. Jour. Sci., (4), Vol. XXXVIII, p. 256, 1914.

¹⁹ Proc. Iowa Acad. Sci., Vol. XIX, p. 149, 1913.

of the period elsewhere. The testimony of the fossils alone, as recently set forth by Savage,²⁰ is very far from being conclusive on this point. A single criterion in correlation is entirely inadequate for the attainment of accurate results, or for the proper correction of the element of personal equation in the broader considerations of the subject. The field especially examined by the author mentioned is entirely too limited for broad deduction. With the Iowa rocks there is no careful comparison; and this is only a small part of the available field. Far to the north over the great Siouan arch traversing southern Minnesota,²¹ and in Manitoba, the Iowa Siluric terranes find representation. Until a greater array of facts to the contrary than those already produced is forthcoming it seems best to parallel the so-called Sexton limestone of northeast Missouri with the Goweran series of Iowa.

Consideration of the diversity of the contained faunas is not without interest. Among the purely physical criteria which may be here mentioned are those of visible continuity, lithological similarity, similarity of lithological sequence, unconformity, simultaneous relations of diverse deposits to some physical event, comparison of changes deposits have undergone from the action of geologic processes supposed to have been continuous, and the paleogeographical relationships. Even the biotic criteria now assume wider bearings than that which takes into account mere identity of fossil forms.

Probably the greatest shortcoming of the various considerations of the fossil faunas characterizing the Siluric rocks of northeast Missouri is the marked insufficiency or lack of breadth in the treatment of the comparisons. This has led directly to the placing of undue emphasis upon the similarity of the faunal elements instead of their essential differences. One result is to throw together into a single limited series beds which other more reliable and mutually supporting criteria indicate as belonging to three great and distinct series covering the whole Siluric Period. The common method of treating individual faunas tends to accentuate the very points which it is most desirable to obliterate. Instead of attempting the mergence of faunal characters it is the emphasis of their diversity which is most needed if exact and permanent results are attained.

²⁰ Illinois Geol. Surv., Bull. 23, p. 30, 1913.

²¹ Proc. Iowa Acad. Sci., Vol. XXI, 1914.

GEOLOGICAL AGE CHARACTERISTICS OF THE COALS

BY JOHN J. STEVENSON

Since the extent of chemical change of rocks increases as a rule proportionally to the antiquity of the deposits the tenor of the fossil fuels is advantageously considered in the order of their occurrence in geological time. A summary account of the accumulation of coal in beds is presented¹ in a recent treatise on the "Formation of Coal Beds." Other phases are included in an essay on the Interrelations of the Fossil Fuels.² Closely related problems have to do with the ascertainment of the relations of the fossil fuels, other than petroleum, in their physical and chemical resemblances and differences.

Peat is the familiar accumulation of more or less changed vegetable matter observed in localities sufficiently moist. It is most abundant in Pleistocene and Recent deposits, but a very similar material occurs in Tertiary formations, and even in the Carboniferous terranes one often finds a substance which in hand specimens can hardly be distinguished from well-dried peat.

The area of Quaternary and Recent peat deposits is apparently greater than on which carbon deposits were laid down during any preceding period of similar duration; yet it is but a small part of the earth's surface, for there are vast spaces on which no peat has formed since the Quaternary period began though much of the peatless regions has been forested.

Peat bogs vary in size from a few square feet to thousands of square miles. The smaller deposits are due to filling of pools, ponds and lakes by plant invasion; while the more extensive deposits, those on coastal, or broad river, plains originated, certainly in some, probably in most, cases in small, isolated bogs, which became united by transgression. These, though continuous superficially, are not strikingly contemporaneous throughout. The buried

¹ Proc. American Philos. Soc., Vol. L, pp. 1-116, 1911.

² Ibid., Vol. LV, etc., 1921.

deposits off the coasts of Holland, Belgium and northern France are continuous with living bogs on the mainland; but the buried peat, in greatest part, is older than that now exposed, as evidently the march crept gradually inland during the subsidence. In like manner, the great deposits formed on plains show notable variation in thickness as well as in composition. The vast peat-overed plains of Alaska and Siberia have contemporaneous top-layer, but the underlying portions of the deposits are probably very far from being strictly contemporaneous.

The condition prerequisite to formation of peat is an abundant supply of moisture, with sluggish drainage; this does not mean that alternating wet and dry seasons are necessarily preventive. If the supply of moisture suffice to keep the main mass moist the loss during dry season is more than made good by growth during the wet season, as shown by some tropical swamps. This condition of moisture depends greatly upon the topography which determines the character and extent of drainage. In the cold regions decomposition is less advanced than in lower latitudes and the accumulation is of vegetable matter rather than of peat properly so-called. The fact is certain that in the tropics as in the temperates peat accumulates where the necessary conditions exist, and that it does not accumulate in either when those conditions are wanting.

Peat may be derived from any land plant; but ordinarily the flora contains many types. The constituent plants vary at the several horizons in a deposit. For the most part the peat does not consist of any one plant or class of plants. Occasionally a layer consists of a single species; but this occurrence is relatively rare. The peat-making forms are not the same in all localities. In northern Europe, and also in some parts of North America, certain mosses are the important constituent in the upper layers; but there are considerable areas in both regions where mosses are either wanting or are wholly unimportant. Sedges have been the efficient peat-producers in much of the north temperate, and even in some tropical and subtropical localities. But there is no limitation; conifers, palms, deciduous trees, mosses, sedges, in a word, any water-loving plant or any plant preferring a slightly acid soil will yield peat under similar conditions, and the soft parts soon become pulp but the harder parts change more slowly.

The felted structure of the peat is not due to any special char-

acter of the plants, for it is present in forest litter. The extent of chemical and physical change increases downward in a deposit. At the top of a growing bog one finds living plants; but within two or three inches the mass consists of dead material, slightly changed in color, but with small increase in percentage of carbon. Lower down the organic structures become less and less distinct, and at length the whole mass is, to the unaided eye, merely pulp, in which are embedded fragments of wood and occasional leaves.

Stages of growth in the peat deposits depend very largely upon the character of the original topography of the area. In the filling of water-basins the first stage is the formation of mud on the bottom. The accumulation of peat has been continuous in few localities; even small deposits show pauses like those which characterize those of great extent. Many times a cyclical order is distinct and the deposit is divided into benches. The benches may pass gradually, the one into the other, or they may define sharply by partings. At times the partings may consist of mineral charcoal mingled with extremely fine mineral matter, the residuum on the surface of the peat long exposed to oxidation. Such partings mark a period of dryness without invasion by forest during which the peat wasted. But partings of clay, sand and marl mark invasions of water carrying detritus.

Expansion of peat deposits by transgression has been observed in all parts of the world. In many deposits of wide extent the fact of transgression becomes evident only after removal of the peat for fuel during reclamation. The effect of pressure on peat is to render it so similar physically to brown coal that the resemblance to the latter is very strong.

Peat contains introduced materials of various kinds. Logs and stumps of trees are not of this class; they are merely the more resistant parts of peat-making plants. Fragments of rock, sometimes angular, sometimes water-worn, have been reported from some localities. The infrequency of references may indicate rarity of occurrence, localization within the peat, or the indifference of observers. The facts available are so few that any suggestion as to origin of these fragments would be worthless. Often there is much silt; at times one finds pockets of sand or clay, and even fresh-water limestone.

The several branches of a peat deposit often differ notably

in mineral content, showing variations in conditions during formation. Bones of mammals, shells of fresh-water mollusks, and remains of insects are of common occurrence. Peat deposits have yielded the best specimens of Pleistocene mammalia; domestic cattle are often mired in swamps; and whole troops of armed men have perished in Scottish swamps during flight after defeat in battle.

Composition of peat depends ordinarily upon its age; that at the bottom of the deposit not only approaches complete disintegration, so that to the unaided eye it shows no trace of organic structure, but it also is far advanced in carbon-enrichment. Yet peat from neighboring localities where conditions seem to have been similar, may show dissimilarity in composition. One finds strange contrasts even in benches of a single deposit, for some may be far advanced, while others consist of almost unchanged plants.

Tertiary coals are known as lignite. The passage from lignite, or brown coal, is extremely gradual. In Europe generally the complex group designated as brown coal is always sharply set off from the Paleozoic, or "Stone" coals, so that, since Mesozoic coals are comparatively inconsequential, the effort there is to ascertain why brown coal and stone coal are so unlike, and to discover reasons why the former could not be converted into the latter. In North America the conditions are entirely different; the coals of all types from wood-like lignite to bituminous, and even to anthracite, occur at times within a single district, in a single bed, or even within the limits of a single estate.

The passage from one type to another is so gradual that chemists and geologists of North America have labored hard and long to discover some means for distinguishing them. The problem is no longer one of mere abstract or scientific interest; it is of the utmost practical importance, since within vast areas the only source of supply is in the Tertiary and Cretaceous formations. The effort is to determine distinctions which will be available for both the seller and the purchaser of fuel. Coal is found in all portions of the Tertiary succession.

Tertiary coals are notably circumscribed, their areas varying from a few square yards to several hundreds of square miles, in some instances apparently even to 2000 square miles. The expanse being limited by the nature of the topography as the deposits appear, for the most part, to have accumulated in shallow lakes or

in well-defined valleys. The lens-like form has been emphasized by all observers in every portion of the Tertiary belts. Unfortunately the details recorded for most regions are insufficient to justify any attempt at working out the history of any bed known to cover a large space. Detailed study is, in fact, impossible at present either in the United States or Canada, where alone the great beds are known, because they occur in districts with sparse population, and where for long distances, one must depend upon imperfect natural outcrops, or on the less definite lines of clinkered rock caused by the spontaneous combustion of the coal. The perplexity is increased by variations in thickness and composition of the intervening rocks, as well as by similar variations in the coal beds themselves, all of which make correlations extremely difficult.

Some American observers, notably Keyes, Bain and Winslow, decline to regard coal deposits as continuous over large areas, but prefer to describe "coal horizons." All agree to the lens-like character of many beds; even those who are unwilling to accept this for the great beds frankly present the frequent changes into shale and the local disappearance of the coal as serious problems. Some observers have shown that the lenses often overlap, that the coal thins out, and may be replaced by another a few feet higher or lower. This feature, so characteristic of American localities, was early recognized by Credner and by Raeffler in the coals of Prussian Saxony.

The intimate resemblance of many brown coal deposits to those of peat has been affirmed by many observers. Collier recognized the resemblance in Alaska and Washington, as also did Eldridge in Alaska; Haast was positive respecting it in southern New Zealand; several authors have described it the Moorand Mooskohle of Prussia and Bohemia; Gothan and Horich have shown that the Torfdolomite of the Lower Rhine is merely mature peat replaced by inorganic matter; Smith and Travers described as peat an impure brown coal underlying the London clay.

The carbon content of brown coal varies. In a general way it gives proof of great advance over peat. Yet in many localities the process of conversion stopped short of the stage reached by most of the fuel peats of which analyses are available. Pliocene coal of Bavaria has from 62 to 69 per cent; Miocene coal from one mine in Bavaria has but 49; the Edeleny coal of Hungary has

but 54; and the Grottau coal in Bohemia has 53; but in other localities in Bavaria the carbon is almost 71; and near Brennberg, in Hungary, it is 72. Oligocene coal in the Gran-Comorn district in Hungary shows 65 to almost 74; the Brandenburg coals have 60 to almost 71; the Zeitz area of Saxony 65, and the Cologne area only 62. Eocene coal of Bovey Tracey has almost 70; coal at Rockdale, Texas, contains 67 to 77 in samples from several mines on the same bed. In the northern areas within the United States there are few analyses showing less than 70, while some reach 75; but in Alaska coal from most of the beds has from 64 to 70.

Beyond all question there is at most localities distinct evidence of progressive enrichment in carbon, with loss of oxygen as one descends the scale. The extremes of carbon content in peat are 40 to 64; in the Miocene, 49 to 72; in the Oligocene, 58 to 70; in the Eocene, 64 to 79. At the same time one must recognize that, as in peat deposits, the progress of change was checked in some localities at a much earlier stage than in others.

Coal of Cretacic age occurs more or less abundantly in many countries. The original areas in which it was formed vary from mere patches to thousands, even hundreds of thousands of square miles; but these greater areas have been broken by erosion into isolated basins, or into isolated fields, sometimes widely separated. The coal seams are not confined to a single horizon, but are present throughout the Cretacic section at localities where proper conditions existed. The several regions have so many features in common as well as so many in contrast that a detailed description of some typical areas, though perhaps tedious, is almost necessary for proper understanding of the relations.

Cretacic deposits are present on the Atlantic and north Gulf coasts of the United States, but they contain no coal; and the occurrences of lignite have interest only for the paleobotanist. The important area is in the west-central region, where the deposits originally extended from the 95th meridian westward for not far from 1000 miles, and from latitude 25 degrees north in Mexico northward for not less than 2100 miles; in all not less than 2,000,000 square miles. These figures are merely approximations; and the area of greatest expanse may have been considerably larger. The continuity of these deposits was destroyed by post-Cretacic erosion following the Rocky Mountain revolution.

Belief that Cretacic deposits were practically continuous throughout this area is of comparatively recent date. The prevalent conception until within a little more than 20 years was that the Rocky Mountains had existed during Cretacic time.

Certain chemical features are characteristic of the Cretacic coals. No substance resembling the pyropissite of Sachsen has been mentioned by any observer, the only allied material being that seen by Dunker in the Hanover region, which he thought might be hatchettin. Resin of one sort or another occurs commonly. By some authors it is termed Bernstein, retinite, walchovite, or simply resin. It occurs in grains or lumps, some of the latter being several inches in size from the lower Quander coals of Bohemia and Moravia. At one locality in Hungary it is so abundant as to give the local name to a coal seam. There is much in New Zealand. In North America resins are characteristic features of coals in the Laramie, the Fox Hills and the Pierre formations, as well as those of the Benton beds. The color is given from honey-yellow to dark-yellow, and according to Thiessen it is rather darker in the Fox Hills coals of northern Colorado than in the Eocene coals of the Dakotas. Resins appear to be wanting in bituminous coals of high grade; at least no note is made anywhere respecting their existence in such coals.

One who reads reports covering extensive areas is liable to believe that caprice has determined the distribution of coal. The presence of coal at one locality gives no assurance that it will be found at the same horizon in others, for great barren spaces exist between productive areas, so that individual seams appear to have small areal extent; apparently the total area on which coal was accumulating at any one time was a comparatively insignificant part of the whole. There is, however, an evident relation between the occurrence of coal seams and the prevailing character of the sediments, which would justify the assertion that in one locality coal may be present and that in another it is almost certain to be absent. The descriptions seem to prove that coal seams accumulate only under conditions such as mark great river, or coastal, plains, where intervals of relatively rapid subsidence were followed by others during which subsidence was slow; finer materials were deposited on the coarser and coal accumulation began. But where the deposits were fine, such as those

laid down at a notable distance from the source of the materials and under a practically constant cover of water, coal is absent.

The relations are sufficiently clear in the Late Cretacic succession of Europe. The immense area of Cretacic formations in the United States and Canada affords ample opportunities for comparisons. Each formation, with the possible exception of the Niobrara chalks, is coal-bearing. The chief source of detritus was at the west, though important contributions were received from the southern border, which probably lay in northern Mexico not far from the international boundary.

In form the Cretacic coal deposits are all distinctly lenses. No statement to this effect is found in any of the older works on the fossil fuels, since nearly all writers prior to less than 25 years ago, held in somewhat hazy way that coal seams were of continuous deposits. Comparison of sections in all fields proves that this conception was erroneous. The Wealden coals of Hanover are local, present in one section, absent in others, and in all cases they have small areal extent. There is a rather persistent coal horizon at the base which seems to be made up of overlapping lenses. The lower Quander district has only nests of coal which occasionally become workable; the Hungarian coals are well defined lenses, as are also those of Queensland; and the detailed studies in New Zealand have proved lens-form in the great coal seams of that region.

During later years the same condition in North America is so marked that it has been noted by the great majority of observers. Occasionally a seam has an area so extensive that the describer is unwilling to commit himself as to the form. But it must be remembered that, even though the lenses have areas of hundreds or thousands of square miles, the general features are the same as those of the smaller lenses united by transgression to form the larger one.

Laramie coals are disposed in lenses usually small and thin within the United States. The great coal-bed of the Saskatchewan region, in Alberta, becomes merely a thin deposit of carbonaceous shale in its southern extension. The Fox Hills seams are lenses usually thin and impure, but locally important and workable over considerable areas. This feature is particularly noteworthy in all districts along the eastern base of the Front Ranges in New

Mexico, as well as in the southern tier of countries in Wyoming. The Middle Pierre or Mesa Verde succession is probably the most productive formation of all, with usually one or more workable seams; but its seams are like those of the newer formations. They are variable and uncertain in New Mexico. In the Uinta Basin, west of the Grand River, portions of the section contain workable beds of coal in some districts but are entirely barren in others; east of that river the coals are local, important here, unimportant there, or absent elsewhere. Mesa Verde coals of the Green River Basin attain commercial importance in only one county. In Montana the lenses are usually small and thin. In Alberta the coals are present in great area and are often workable, but available details merely suggest that they are lenses.

Coal lenses ordinarily show increase of foreign matter towards the borders. The seam becomes broken up by fine partings, and very often it passes at last into merely impure carbonaceous shale with thin laminæ of coal. Sometimes the lenses are connected by a stretch of black shale; but commonly no such bond exists, and a barren space intervenes. These lenses, great and small, are similar to the peat deposits on the broad river plains, and even more strikingly resemble those on coastal plains. At times these are separated by broad spaces which are forested. At other times they are united by carbonaceous muds; while in still others the peat of several lenses has become continuous by transgression.

In all coal-mining districts the effects of contemporaneous erosion are conspicuous. Curious intermingling of coal and débris, observed at one locality in the Loewenberg area of Silesia, seems to be explicable only on the supposition that it represents a washed-out swamp. The presence of coal grains in certain sandstones may signify that a coal seam in process of formation was exposed. Local conglomerates in many sandstones occupy old channel-ways of rapid streams. Local unconformities between sandstones and shales suggest changes in direction of drainage. Coal seams themselves appear to have been often subjected to subaerial erosion, and to have been frequently traversed by streams as in modern swamps. "Horsebacks," or rolls, of the roof have been found wherever extensive mining operations have been carried on. They mark channelways of varying width and depth, now filled with materials like that of an overlying deposit. Sometimes the material is the same as that forming the immediate

roof, in which case the stream was probably contemporaneous with the bog; but not infrequently the channel-way was excavated after the roof had been formed. The conditions are common-places in modern deposits.

Cretacic coals are usually so far advanced in conversion as to give little information respecting the plants of which they were formed. Knowledge of the flora of the period is mainly derived from fragmentary materials found in the associated rocks. It has been transported; and represents mostly the upland vegetation, telling nothing about the swamp plants. In the United States and Canada Cretacic coals are often rich in resins, indicating that conifers entered largely into their composition; such wood as has been recognized seems to confirm this conclusion. Cycads were abundant locally during the deposition of the Kootenai beds, but conifers and dicotyledons were predominant during the Late Cretacic times while ferns and lycopods appear to have been subordinate. European information upon this subject is scant. Wood, clearly recognizable, is present in the Late Cretacic coals of the Loewenberg region; but in the Grünbach coal no structure is shown, although the stems and branches retain their form. The Wealden coals of Hanover contain abundance of conifers, cycads, lycopods and ferns. The plant remains must be very distinct there. Dunker thinks that the "black coal" of that region was derived from lycopods and ferns exclusively because they are the only forms found in it; the lignitic coal is largely of conifer origin, since the stems occurring in it resemble those of *Pinus*.

Consideration of the chemical relations of the Cretacic coals has to be taken in connection with those of the older coals. Certain features are of special interest. Like the Tertiary coals and some peats these coals are resinous in many districts. Cannel coal is present at several horizons. Carbon content is higher than in the Tertiary coals; but progressive enrichment with increasing age is less marked. In the Fox Hills coals the extremes of carbon are 73 and 83 per cent.; in Pierre, 71 and 84; in the Benton, 77 and 83; and in the Kootenai, 75 and 85. No note is taken of the metamorphosed coals; and anthracite is present at several horizons. No ultimate analyses of the Laramie coals are available, and there are very few of the Kootenai. Variations are small compared with those of the Tertiary coals. Among the Cretacic, as in the

Tertic, coals not all the accumulations of vegetable materials had attained the same degree of enrichment before burial. The minimum for the Pierre analyses seldom falls below 75; yet there are some seams with only 71 or 72. The peculiar condition is well marked in the Hanover district where the "black coal" has 89 per cent. of carbon, the brown coal 73, and the Blätterkohle is almost unchanged, yet the several types occur in the same vertical section.

Jurassic and Triassic areas containing coals in economic quantities are utterly insignificant when compared with those in which the rocks of these ages are exposed; but there are many localities in which coal materials accumulated during brief periods and amid unfavorable conditions. Oolite coals of Britain and the few spots on the continent of Europe are of inferior quality, and are merely local and without especial interest. Elsewhere the useful deposits are in the lower part of the Lias formation. The Jurassic succession above the Lias horizon and the Triassic below the the Keuper formation may be regarded as barren.

The strata associated with the coals are similar to those displayed in the later periods. The Oolite coals of England are intercalated in sands. The Jurassic coal beds of Spitzbergen are confined to the Middle, or Sandstone, division as defined by Nathorst. The Grestener, or coal-bearing Lias of Austria is composed of sandstones and shales. Similar conditions prevail in the Liassic coal areas of Hungary and Siberia. The Jura-Trias section of Queensland and New South Wales is almost wholly sandstone.

The Late Triassic section in Austria and Hungary is sandstone mainly, with some intercalated shales. On the other hand the Jurassic coal bearing rocks of Alaska are almost entirely shales; the Late Triassic deposits in some small areas have little sandstone.

Freshwater fossils in rocks associated with coal seams have been observed in England, Siberia, Spitzbergen, France, and Queensland. The structure of the rocks is evidence of, at most, shallow water; and in some cases it is very suggestive of eolian agency. False-bedding is reported from England, Australia, Germany, and North Carolina; and ripple-marks are common features at many places. Sandstones and shales frequently contain logs of wood, in such relations as to leave little room for doubt that they were simply stranded materials.

There are ample proofs that the sea invaded many localities where coal was accumulating. The Lower Oolite of England has beds with great abundance of fragmentary marine shells. Liassic sandstones of Austria and Hungary include layers containing many forms of marine mollusks of littoral types. Ammonites are found at one locality, but that does not militate against the conclusion that the water was shallow. If the shells be not drifted it shows that the genus could exist in shallow water. In any event, these deposits suggest that the areas in which they exist were lowlands close to the ocean level.

Paleozoic coals require special consideration. In a great part of the areas where the deposits of Permo-Carbonic age are exposed the passage from the Triassic sequence is gradual; at most, the plane of union shows only petty discordance of stratification. In many extensive areas the succession of terranes is incomplete and some members are missing, so that the Triassic beds rest on any formation from the Archean to the Permian. In like manner when the rock sequence is complete the Permian beds may pass downward into the distinctly Carbonic strata so gradually that no definite boundary can be determined stratigraphically or by aid of changes in plant or animal remains. At times deposits assigned to the Permian rest on pre-Carbonic rocks, while in vast areas the succession is apparently conformable throughout. Lithological changes usually occur in the upper part of the section. At one time the presence of red coloration of the rocks was considered proof that the Permian beds had been reached. This opinion is not final, since in many regions red beds occur in distinctly Carbonic strata. Frequently the basal portion of the Permian section contains conglomerates, holding pebbles which are striated seemingly by glacial action.

The problems concerning the relations between Permian and Carbonic coal measures are vexatious to the last degree, since the testimony of stratigraphy, paleontology and paleobotany seems to be conflicting. In some cases the conflict is not real; but in others it is a fact and it can only be removed by revision of conceptions which have become laws because accepted for a long time. However it is unnecessary to enter into discussion as to the propriety of regarding the Permian division as more than a subordinate section of the Carbonic succession.

GEOLOGICAL WORK OF R. ELLSWORTH CALL

By CHARLES KEYES

Recent announcements by the press of the demise of Prof. R. Ellsworth Call brings to mind the fact that this scientist was long numbered among our fellow geologists of the country. Although Call was one of those all-round naturalists of the old school now fast approaching extinction, and was widely read in all scientific realms his efforts in the domain of geology were not inconsiderable. He was really one of the recognized secondary geologists belonging to the last quarter of the last century.

A most versatile genius and one as erratic as the famous Rafinesque whose life he took such keen delight in portraying, he explored many fields. While he had broad general knowledge of animals and plants and minerals he knew much about particular types and circumscribed groups. Recognition as a foremost authority on mollusks did not deter him from delving extensively into other branches of zoology, not from exploring diverse departments of earth history. He was one of the first men elected to fellowship to the Geological Society of America. Withal he was an ideal and inspiring teacher of the science and he was especially popular as a lecturer before young people and mixed audiences.

I knew Call well. His family lived in Des Moines. For a period of many years even while his employment took him into distant states, he usually spent the summers with his parents. During this time his extensive collections of shells and his fine library were housed in the Capital City. He was the author of many memoirs, mainly upon subjects relating to Conchology.

Richard Ellsworth Call was born on May 13, 1856, in Brooklyn, New York, where his early education was obtained in the public schools. After grammar school he attended the Cazenovia Seminary from which he was graduated in 1875. From there he went to Syracuse University, but did not remain to finish the prescribed course. While there he fell in at Ithaca with David

Starr Jordan, John Casper Branner, and other Cornell scientists of that day, who in after years were often closely associated with him in his investigations. Leaving college he taught country school for several years, at the same time devoting his spare hours to collecting and studying in natural history. In the meanwhile his parents moved to Des Moines. During the years 1890 and 1891 he attended Indiana State University, receiving the A. B. and A. M. degrees. In 1893 he finished the medical course at the Hospital College of Medicine of Louisville, Kentucky, graduating with the degree of M. D. Ohio University, at Athens, conferred upon him the honorary degree of Ph. D. in 1895. He died in New York City on March 14, 1917.

Call's principal avocation was teaching. He was connected with the schools all his life. Besides instructing in the sciences in the high schools of Stonington (Connecticut), Moline, Des Moines, Louisville, Brooklyn and New York City, he occupied for a time the Chair of Zoology in the Missouri State University. He served as curator of the Brooklyn Institute of Arts and Sciences; and for periods of three years each he performed the duties of superintendent of Public Schools of David City, Nebraska, and of Lawrenceburg, Indiana. He was an able and entertaining lecturer, and his services in the field were much sought. His work as lecturer for the Board of Education of New York City was especially noteworthy and satisfactory.

Of his more productive work in pure science there was wide range. In geology his efforts were mainly of the reconnaissance type. Yet he published a number of geological memoirs of note. Joining the staff of the United States Geological Survey, he spent a long season in the deserts of Utah with Gilbert, who was then studying the old desiccated Lake Bonneville — the all but vanished remnant of which is the Great Salt Lake of today. From the clays and sands of the old beaches of that vast ancient body of water he collected the molluscan shells, endeavoring to show by their depauperate character that they lived under the inhospitable environment of a glacial climate, to which Gilbert ascribed the origin of this great expanse of inland waters. Some of the forms unearthed proved to be new to science, and were so described.

After similar fashion he worked with the late W J McGee on the loess and the loess fossils of central Iowa. Notwithstanding the fact that depauperate shells were found to be abundant at

Des Moines, in other parts of the state that they were not detected by subsequent investigators. Later, also, the loess itself was demonstrated to be not a deposit derived from the glaciers but a warm-climate interglacial formation. However, together, McGee and Call barely escaped making one of the great geological discoveries of the century — the establishment of the multiplicity of the Glacial Epoch. The Des Moines sections furnished all the evidence but they were not properly interpreted.

Call afterwards extensively studied the loess of the upper Mississippi valley, and come to the conclusion that this remarkable loam formation was a great lake deposit, accumulating something after the manner of the beds of the alleged vast Tertiary lakes of the Great Plains region. Here again his judgment was at fault, for all of the deposits of this description were finally proved to be mainly epirotic formations laid down by the winds. His close association with government folk in the Far West evidently firmly implanted in his mind this early but erroneous notion. His views on this subject are elaborated in a series of articles which appeared in the *American Naturalist*. These papers were long worthy of careful perusal if for no other reason than that they supplied the best summary of our scant knowledge on the loess up to the date of their publication.

Call's monograph on the "Mammoth Cave of Kentucky" was a huge quarto tome, edition de luxe, sumptuously printed on deckle edge, antique wove, unsized paper, and contained 30 plates. Its character was historic, scenic, biologic, and bibliographic.

During several summers Professor Call served as assistant geologist on the Arkansas Geological Survey under Dr. J. C. Branner. His main work was on the Crowley ridge, a long narrow elevation of Tertiary formations rising out of the wide Mississippi flood plain in the eastern part of the state. The report was published as a special volume of the Survey series.

When resident of Iowa Call became interested in the artesian waters and collected data of a number of deep drill-wells put down in various parts of the state. His principal results appeared from time to time in the *Bulletin of the Iowa Weather Service*. Many brief papers and articles were published on geological subjects in the scientific journals.

Call published most extensively on the mollusks, fishes, and reptiles. A synopsis of the Unionidæ of the United States formed

the initial number of a Bulletin series of the Des Moines Academy of Sciences and found wide circulation. His shorter papers on Conchology were many and varied. The "Anatomy of *Campe-loma*" was a model of its kind, and was based entirely upon materials obtained around Des Moines.

The Unionidæ of Arkansas formed a large illustrated memoir which was published by the St. Louis Academy of Sciences.

One group of mollusks in which Call became very much interested was the little known family of the Strepomatidæ, turreted snails inhabiting southern rivers. The Coosa, Black Warrior, and Tombigbee rivers of Alabama in particular harbored these water snails. For a number of years he was accustomed to collect extensively in these streams and their numerous tributaries. Isaac Lea, Thomas Conrad, Thomas Say and Constantine Rafinesque described many species but these were never very well defined; and a large synonymy resulted. It was our Iowa naturalist's especial mission to pass in review all the described forms, to collect abundantly from all the original localities, and to adjudicate the numerous varieties in accordance with modern canons of taxonomy. This he was able to do in most satisfactory fashion. Having accomplished this gigantic task he generously sent typical and authenticated sets of the shells to many of the principal museums of this country and Europe where conchology was stressed. Many private cabinets were also made beneficiaries of this work.

Investigations on the fishes were mainly systematic in character. Part of the time spent along these lines was in conjunction with Prof. Seth E. Meek. He made a very complete collection of the fish fauna of the Des Moines river basin, which for some reason was never quite finished or published in full. A preliminary account appeared in the Proceedings of Iowa Academy. He worked for several years on the fishes of New York. How complete this work was at the time of his death was not known. Much was done towards working out a better taxonomy of North American fishes. In a similar way he was intensely taken up with improving the taxonomy of North American reptiles.

The "Fishes of the River Ohio" was a magnificent volume, and Call's most complete work on the group.

In the field of botany important contributions were made to a knowledge of the hardwood forests of Arkansas, the ferns of the Ozarks, and the plants of Iowa.

When residing in Louisville, Call unearthed, among the historical documents of the Filson Club of that place, the unpublished notes of the eccentric French naturalist, Constantine Samuel Rafinesque, who for many years in the early part of the last century made America his home. In the "Life and Writings of Rafinesque" Call set aright most of the old naturalist's descriptions of invertebrates of the Mississippi valley, which had long been the despair of systematists of later days. This sumptuous quarto volume was published by the Filson Club, and proved to be one of its most cherished publications.

Call's was really a brilliant mind. Had he been set in a congenial environment and had he not been continually hampered by his teaching, which he was always forced to follow in order to gain a livelihood, he doubtless would have developed into one of the great naturalists of his country and perhaps of his day. His purse was always lean; and he could do little along purely scientific lines that he planned. Although genuinely generous many of his actions were often misinterpreted by those who did not know him very well. So preoccupied was his mind at times that he became very forgetful. Not infrequently he would borrow an armful of books from some friend and the very next day he could not for the life of him tell to whom they belonged. On this account some of these books doubtless never got back to the original owners. It was the same with specimens. Soon many persons began to judge him harshly. Really this was largely mistaken inference. On the other hand he was equally careless with his own property. Lending freely any of his books or specimens he promptly forgot by whom they were received; and it might be months before they turned up again.

These things changed greatly after his marriage, which took place rather late in life. His absentmindedness grew noticeably ameliorated. At the same time his powers of concentration of mind visibly deteriorated. His productive efforts became less spontaneous and more irregular. Within a lenstrum he ceased publishing altogether; and soon passed out of sight of the associates of his old scientific circle. From that day to the date of his demise, twenty-five years later, he remained completely inactive; and the newer generation of zoologists knew him not.

The experiences of the Iowa Academy furnishes a curious instance of his usual lack of mental equipoise. The minutes of the

early meetings which were turned over to him as secretary on about the third or fourth session immediately vanished. He had laid them down somewhere while the members were chatting after adjournment. In his endeavor several years afterward to record the proceedings in a special blank book purchased for the purpose he lost two entire meetings, forgot the titles of half of the papers read, and failed to enumerate most of the charter members and original promoters of the society. Several years later when the State of Iowa assumed the publication of the Academy's Proceedings Secretary Osborn did all he could to rectify these delinquencies by obtaining from each member abstracts of his papers and printing them *en masse*. In this way some members provided notes on no less than six to eight papers which they had actually presented and read but of which no record had been made in the minutes. Therefore Professor Pammel's account of the charter members given at the twenty-fifth anniversary of the founding of the Academy omits many important facts and details because of the fact that he relied too implicitly upon the faulty Call record.

In the late eighties of the last century a number of Professor Call's friends in Des Moines, realizing fully both his brilliant attainments and his difficulty in getting a suitable living, put forth special effort to have him appointed to the headship of the science department of the Des Moines West High School, then a much sought post. In this they succeeded nicely; and he entered upon his duties with great zest and high hopes. It so happened that about this time he was intensely interested in the reptiles of the state. He soon had his entire body of students enthusiastically studying snakes. All else in science was neglected or forgotten. Students even slighted their other duties to attend the Call snake classes. In order to take adequate care of the material which fast came pouring in, Call, to cap the climax, invested his entire large allotment of funds for science apparatus and equipment for the year in glass snake-jars and alcohol. Soon the science laboratory would have done credit to the National Museum as a snake repository. Then, one day came wide and emphatic protest; and finally the unfortunate and unavoidable flare-up between instructor and school committee. The versatile and enthusiastic naturalist was soon worsted. The best and most entertaining lecturer the school had ever had, the most ardent scientist who had

ever ventured to the city, and perhaps the best science teacher who ever darkened the doors of high school was summarily dismissed. But his students, with greatest enthusiasm and keen appreciation far beyond their years, had entered the fairy demesne of science. Some of these "delinquents," from that specialized beginning, followed the paths so auspiciously opened up and made science their life's occupation. Perhaps after all this intensive study of a circumscribed field was the best science training possible. *Quien sabe?*

Professor Call was my first acquaintance with a real live scientist of national reputation. It was very early in my career. As a youngster of thirteen years, in the first years of high school, I had already made modest beginning at collecting shells, insects, birds and minerals. My teacher was John W. King, who was also principal. King was notably eccentric in his manners and methods, but he was an avowed follower of Herbert Spencer, and he was especially fond of trying out the Spencerian theories of education. So soon as he found out that any one of his pupils had become especially interested in any particular subject he at once set about to encourage him to greater and more systematic effort. Being a neighbor of Call's he made arrangements to take over one evening half a dozen of his kiddies, among them also Uly S. Grant, who has since become a distinguished authority on geology, and a leader in higher education, as dean of Northwestern University.

Upon our arrival at his home Call joyously gave up the entire evening to these youngsters, showing them his books and his cabinet of shells, all the while giving a fascinating running talk on the high points of interest. The youthful company had also thoughtfully come prepared; for they had their pockets full of specimens of which they wished to know the names and about which they thirsted for more knowledge. They had already learned the long Latin titles of some few forms but they wanted to enlarge their scientific vocabulary. Call willingly helped to do this.

Among our shells were a couple of snails which he said were very rare and of which he had in all his numerous trips in Iowa come across only a single individual; and he asked if he might have one of the pair. Both were instantly put at his disposal much to his evident satisfaction. Then he inquired where we had

found them. On being told he asked if on the morrow we would go with him to the spot. We were of course overjoyed to do so. So, the next morning we led him five or six miles up the river and stopped before a large shrub where we began picking off the snails much as one would blackberries. In the course of half an hour we garnered about two quarts of these shells. Collection of a thousand specimens of this rarest of North American snails Professor Call opined was a very good morning's work. The rarity of this mollusk suddenly ceased. Its *optimum habitat* had previously been miscalculated.

On another occasion I had collected a basket full of river mussels. Among them chanced to be four specimens over which so soon as I showed them Call went into ecstasies. They proved to be a very rare *Unio Wardii*. Of course I at once divided with him, as he had only a single example in his collection. It was immediately decided that we should go on the morrow to the place where I had obtained them and look for others. Arriving at the spot we soon secured about thirty or more. In cleaning mine I noticed that one of them contained two large and very beautiful pearls. A naturalist's delight at finding a *rara avis* vanished instantly with me, and the baser instincts of the savage, which are said to be the heritage of every youth, came to the surface in the kiddy. Leaving friend Call to content himself with the rare shells I went after the gems, to which he paid not slightest attention. Before starting for home I had secured more than fifty fine pearls of various sizes.

On reaching the city I found the local jewelers somewhat reluctant to purchase such an assortment of gems from a strange boy. But the youngster, reinforced by dad, easily disposed of them at what seemed to be very good prices. When finally the last pearl had been parted with and something over a thousand dollars were added to the high-school lad's thin exchequer dad suddenly visioned great financial prospects ahead for a son of his who could wrestle such sum out of home river sands in a single morning. Call willingly and joyously passed it all up.

Several months later when the Des Moines Academy of Sciences, the forerunner of the State Academy, endeavored to raise funds with which to publish its first proceedings that same high-school lad took keenest delight in having the chance to help finance the project by contributing \$80.00 of his pearl money or

one-half of the necessary amount for the printing, Professor Osborn, of Ames, furnishing the other half. So early had kiddy inculcated desire to advance knowledge.

In the spring and summer of 1884 Call was engaged at the Smithsonian Institute, in Washington, in preparing government displays for the New Orleans Exposition. The assemblages there of mollusks of the country were especially fine and novel, and occasioned wide and favorable comment. One of his letters at this time to me a youngster was particularly characteristic of his boundless enthusiasm in all matters relating to natural history. In the midst of his rush he found time to write:

Smithsonian Institution

My Dear Boy!

Washington, 22 July, 1884.

I am writing you in a hurry to have you perform a mission for me; and help yourself. I am, as you know, arranging the fresh-water and land shells of this country for exhibition at New Orleans. Iowa shells are poorly represented. Now, I want you to go to the Des Moines and Raccoon Rivers, and get ten of each kind of *Unio*. I don't care how common they are. Clean nicely and ship to Smithsonian Institution, care of R. Ellsworth Call. I will do all the labelling; and place them in the collection credited to you. Send the very largest and finest of each kind you can. Take a couple of weeks to the collecting, for I wish our old "stamping ground" well represented. Will you do this favor? Any small shells you may find send along—such as *Amnicola*, *Succinea*, *Mesodon*, etc. You will receive an equivalent for them in due course of time. I would like fine *ventricosus*, etc. Take the shells as little eroded as possible, and the prettiest rayed. You will find your work appreciated, I can assure you. Go also to the Des Moines River by the C. B. & Q. RR. bridge in the city.

What did Grant get in the Northwest? Write me.

Faithfully yours,

R. ELLSWORTH CALL,

"Dear Boy," probably with vanity deeply touched at the time, but long since forgotten, and with huge State pride, memory of which still lingers, gladly spent the best part of a week in getting Iowa natural history adequately represented for the Exposition. A collection of one hundred and fifty species comprising upwards of two thousand specimens was forwarded in several large boxes.

At the time little thought was given to the possible return exchange mentioned in the communication. However, some months afterwards, several heavy mail sacks were delivered to the young shell collector in Des Moines, much to his surprise and joy.

They contained half a hundred books on geology and natural history — publications of the Smithsonian, the Geological Surveys, and Government Exploring Expeditions. Accompanying them was a cordial personal letter from the distinguished Secretary of the Institution, warmly thanking me for the fine collections. This parcel of books at once formed the nucleus of "Dear Boy's" library, which grew apace with the advancing years, and today, forty years after, still holds honor place in his den.

Years later, when visiting the Smithsonian one day, Secretary Walcott called me aside and led me to a display of shells which he had noticed a few days before — Iowa shells all with my name attached. They greeted me after half a century as old friends. But their guardian spirit had already passed to his reward.

Call was indeed a naturalist of the most versatile type. This very fact prevented him from concentrating effort deeply upon any one thing for any length of time. His exceptionally alert mind and normal great activity thus largely spent their force unavailingly. His efforts were bent along the line of the formal systematist rather than that of the philosopher. Within him product rather than process was the all-important desideratum. He was widely read; and of biological topics his knowledge almost bordered on the uncanny. There were few fields of science in which he could not discourse intelligently and at length in all their genetic, developmental and taxonomic aspects.

He was what is generally called a Bohemian, although always with serious ambitions. He was brilliant talker whether in a small company or on the lecture platform, fully able at the moment to turn his vast knowledge to account. His conversation abounded in lively anecdote told with infinite zest; he was thoroughly genial and ready at good humored repartee; and he was never hampered by any excessive reverence for ancestral proprieties. Even an ordinary social gathering must have consisted of very ponderous interests if it could not be stirred into animation by a man with so much more quicksilver in his veins than falls to the lot of the average citizen.

Call was generous to a fault, helpful beyond measure, and thoroughly sympathetic. As a teacher he was seemingly without a peer. It was perhaps from this angle that the value of his great services should be judged rather than from that of cold, copious and creative productivity.

EDITORIAL

PASSING OF A VENERABLE MINING INDUSTRY

Recent closing down of the last operating Cornish tin mine in England is one of the melancholy events of the ages. For a period of more than forty centuries these mines continuously furnished the chief supplies of tin for the world. Perhaps a prosperous industry when the Egyptian pyramids were building, it is probable that the early Iberians who migrated up the west coast of Europe from the Mediterranean region first knew of these deposits and carried back the glad tidings to their civilized homeland. It may be that through these last mentioned adventurers tin was early made known to the Egyptians and that they discovered the means and the art of working bronze a millenium or two before the Phœnicians arrived upon the scene.

In the history of civilization the discovery of tin was indeed revolutionary. Although bronze was long centuries getting into Britain, Mediterranean civilization made regular expeditions there after the precious tin. Phœnician ships traversed the entire length of the interior sea, passed through the gloomy Gates of Hercules, and braved the dark and stormy Atlantic in order to reach this farthestmost limit of the ancient world — the true *Ultima Thule*. After the Phœnicians the Greeks continued for centuries to voyage to the Cassiderites, or Tin Islands.

Where else in all the world may any other mining industry boast of such unbroken prosperity during 6000 years?

By the extinguishment of the Cornwall mines England loses her most venerable industrial institution. A most famous mining region is no more. As a distinct mining clan Cornishman vanishes from earth. What important mining district on our globe is there that does not know and fully appreciate the great worth of the Cornishman as a miner? Where has he not acted as pit-boss, foreman, or superintendent and not made of the enterprise a success? With tearful eyes we look upon his solemn passing.

The fact that a hundred thousand miners and their families are now starving in Cornwall is not the saddest feature of the whole affair. With strong heart Cornishman will take up his *Penates* and find living in other clime. The end was long foreshadowed. For many years tin-mining in England was on the decline and unprofitable. The recent lowering price of the metal, the war-high price of coal, and other necessary supplies, and the constantly diminishing tenor of the ores effectually combined against adequate financial returns.

Unlike the coal miners the tinnerns never demanded exorbitant wages. In marked contrast and with keen business sagacity many of the Cornishmen, appreciating the desperate straits of their ancestral industry, rose nobly to the occasion and returned to the operators thousands and thousands of pounds sterling of their wages in vain effort to keep their industry alive.

Noblest miner of them all is Cornishman!

ORIGIN OF THE OLDEST FOSSILS

Because of the fact that at the beginning of the Cambrian Period life on our globe appears to have burst forth already nine-tenths differentiated it does not necessarily follow, as inference is sometimes made, that the span of organic existence reaches backwards a ten-fold distance into the abyss of time. Sensational claim of this sort emanates not from biologist or paleontologist but from those who seem to have little familiarity with either. When viewed as an unanswerable realization of the immensity of geological time it is a fascinating theme that the conception opens up. Measurement is made in billions of years instead of the millions or thousands of early calculations. In projecting this vast sequence of the years basic factors are entirely overlooked; but their calm consideration effectually curbs the flights of petrologic fancy.

Students of the immeasurable past learn that the passage of life through the ages is not a simple, even, undisturbed course; but that there is rhythmic expansion, as it were. Short stretches there are wherein evolution goes on by leaps and bounds, and progress is more rapid than ever before or ever afterwards. Advancement in brief epochs is sometimes greater than in the millions of years that lie between. The sensational intellectual development which has taken place since Mid Tertiary time, and the

abrupt changes which have transpired when the land faunas and floras were established are particularly noteworthy.

A much earlier and probably an infinitely more important era of rapid development in the forms of animal life doubtless occurred immediately after the latter became established on the bottom of the ocean. This basic factor was first pointed out by the late Prof. W. K. Brooks, something over a generation ago, when in scientific circles, life before the Cambric Period was just beginning to attract marked attention throughout the world. The Brooks essay is one of the master contributions to evolutionary philosophy. Although written by one who was not a geologist himself, for the contemplation of his paleontological confrères and for that reason published in a geological journal, it naturally failed to elicit the curiosity it really deserved from earth-students. For the same reason it missed its high mission among biologists. Despite this circumstance it merits long and deliberate analysis by students of ancient organic remains and by geologists, most careful perusal. It severely fixes farthestmost limits to oldest fossils.

Brief review of the circumstances which gave birth to this remarkable meditation is not without interest. Possibility of the existence of a thick pre-Cambrian succession of sediments was occupying the center of the geological stage both in this country and the world. Under the title of Algonkian the Federal Geological Survey had proclaimed with great *éclat* the recognition, beneath the known Cambric section, of an entirely new system of sedimentary rocks which, it was hoped, would prove to be comparable to Murchison's and Sedgwick's notable discoveries, half a century before, of the Siluria and Cambria in England.

In this connection the crystalline complex of the Piedmont Plateau, in Maryland, where our zoologist was homed, was under especial surveillance. It was fondly expected that some of these rocks which had always been considered as among the very oldest parts of the Archean *massif*, if not actually a section of the primeval crust, might prove to belong to the so-called Algonkian sedimental column. At this very time under the ægis of the Johns Hopkins University, at Baltimore, these crystalline schists were subjects of intensive study. Some of the results published started wide and warm discussion throughout the world.

With this astounding setting before him, that at the beginning of Cambric time biotic types were already so widely and funda-

mentally differentiated, and the further anticipation of great extension of the life record into time before Cambrian, the nature of the pre-Cambrian life at once attracted the attention of Prof. W. K. Brooks, foremost zoologist and evolutionist of his day.

The bearing of the Brooks disquisition is far-reaching. It furnishes a basis for mathematical evaluation of geologic time more accurate than ever before recognized. It accounts fully for the apparent sudden outburst of life in such grand profusion at the beginning of Cambrian time. At that date the span of evolution was admittedly twice or thrice as long as from the commencement of the Cambrian age to the present. It may have been somewhat longer, but certainly not eight to ten times longer, as is so often urged of late.

Since pre-Cambrian sedimentation is now represented by two great eral successions, with organic remains unearthed far down towards the bottom of the lower, or Archeozoic, division, it leaves a theoretical third life era of like duration in which to permit the primitive expansion of the main animal types. This ancient life era we may for convenience designate as the Eozoic Era. Our standard life column is now as complete as it is perhaps possible ever to construct.

GEOLOGICAL SCIENCE AND THE STATE

Following great war reconstruction days are unhappy days. Sore trials and myriad tribulations which inevitably follow mark not alone industrial activities. Insidiously they sooner or later penetrate every branch of human endeavor. First in government circles, soon in the industrial world, then in the various lines of business, and finally on intellectual productivity they operate. In the wake of business depression and deflation follows deflation and lack of support in the higher enterprises of the mind. Special taxes make themselves burdensome throughout the land and the world. And this in turn pinches activities that they should never touch; but touching, it requires heroic measures to ward off their blighting effects. Although the bite of tax is merely a symptom rather than the disease, it invites, like a headache, first local relief, despite that this does not reach the true cause of disturbance.

In public affairs the symptom is reflected in many ways. Among institutions of learning and state scientific bureaus first attempts at the general relief from the immediate pressure appear in the

legislative assemblies. Strenuous effort on the part of many legislators, who play to political galleries, under the misapprehension that this is real statesmanship, is made to wave on high the flag of economy. In accomplishing this, scant consideration is given to merit or necessity. Anything goes. As in the jungle, only they who survive are fittest.

Under such untoward conditions it behooves every educational and scientific organization to trim its sails to meet the oncoming storm. Our geological surveys are likely first to feel the chilling blast and learn of the natural consequences of national deflation. Trouble already brews in many quarters. Adequate precaution cannot be taken too early if science is to be properly subserved.

Iowa is the first state, perhaps, to feel the new conditions; and she probably will be the first to overcome them. Fair introduction to the *modus operandi* is at this very moment before the scientific people of this state, although the meeting of the Legislature is still a full twelvemonth off. In this community the farmers' organizations have a permanent committee called the Legislative Council. During several past legislatures no important bill introduced was enacted into a law without this Council's endorsement. The political power of this protective Council is measured somewhat by the fact that it is composed of representatives from 35 strong state organizations embracing over 100,000 members. The Threshermen's Association alone contains a membership of 7000, and represents a backing capital of more than \$200,000,000. In many other states similar organizations also exist.

Now, for several years, the Farmers' Legislative Council, after careful investigations of conditions, has not been satisfied with the running of not a few of the state institutions and state bureaus. The Geological Survey is only one out of a large number which has been passed in review, and by it found wanting. But the Council waits not for the convening of the Legislature in all cases. It sometimes goes directly to the governing boards and presents its findings and its recommendations. A short while ago it had the Governor of the State call the members of the Geological Board together to consider in public hearing the objections to the present conduct and alleged shortcomings of the Survey. It is reported in the press or verbally, and seemingly authentically, that nearly one hundred charges had been previously filed with the Governor, and that Professor Kay, the State Geologist, with small show at

either tact or diplomacy treated them with curt resentment, which of course in the minds of the public was not defense but substantiation without further debate. The whole affair is certainly most unfortunate.

Ye editor does not wish at this time to comment upon the merits of the situation; and Professor Kay has his full sympathy in this hour of grave tribulation. But he cannot refrain from asserting, what he has already always advocated, that a geological survey has no business to be tacked on to any college or other state institution and thus be made to share in their troubles. It is to be hoped for the good of geological surveys in general and the Iowa Survey in particular, and our science in its entirety, that in the interest of the higher aims of geological science Professor Kay will rise to the occasion, exchange the petty stipend as honorarium for his summer vacations, and propose some one for the post who is willing to give all his time and energies to the upbuilding of geological interest in the state. It is the earnest desire that the University, as the head educational institution of Iowa, will also nobly rise to the occasion and pay Professor Kay a full professor's salary, one commensurate with the dignity of such position, so that he may not be compelled every year to enlarge his exchequer by petty additions from the outside.

To the very great credit of the Iowa farmers, be it said, not the slightest hint was dropped against the Survey as a public institution. Objection was entirely to the director, and to the charge that he had done so little in the ten long years of his office, after omitting what was left over from the Calvin régime. As one Patriarch tersely remarked, if the Survey is worth while, it is worth having a good man put in all his time at it. So saith report.

One most unfortunate angle which the Iowa Survey finds itself in in its hour of great need is that it had previously alienated the affections of many, if not all, of the geologists of the state. To intensify an already bad situation it had recently proclaimed a ukase which virtually amounted to a prohibition against the publication of anything appertaining to the geology of the state without first obtaining the Survey's full approbation; and judging from certain articles permitted to pass censorship it seemed also necessary to get affixed the official label of "Published by permission of the Director." To many it seemed as if the Survey was striving to minimize the appearance of its own lack of productivity.

All surveys should take warning before the weather gets bad.

STRUCTURAL GEOLOGY

Vanishing of Eastern Coal Seams in Indiana. In the early reports on the geology of the state frequent mention is made of some remarkable "cut-outs" in some of the large coal seams by Paleozoic rivers. A so-called Carbonic river that received special notice is that supposed to have existed in western Indiana, in the vicinity of Coxville, in Parke County. This old stream was thought to have excavated its channel in the top of the Pottsville series, and the evidence of its course to be indicated by a deeply filled valley penetrating the thick seam known as Coal III.

In a recent special examination of this region the deposits occupying the line of the old alleged channel were found not to be filling a depression at all, but to form a distinct ridge. Coal III of the Alleghany series was demonstrated to thin out gradually to a knife-edge against the Coxville ridge. Over this ridge Coal III appears never to have accumulated in its full thickness.

On the other hand the rider seam, Coal IIIa, was deposited in full thickness over the elevation and under normal conditions.

The narrow belt of sandstone formerly thought to fill a deep erosion channel does not therefore cut out the great coal seam but represents a ridge in the ancient swamp basin which however appears to have disappeared before the deposition of the rider.

The mining significance of this fact is of more than local interest, because of the circumstance that in so many localities throughout the coal fields old river channels have been described, whereas some of these alleged gorges are not such but perhaps even sand-dune formations instead. Instead of thick coal seams holding out in full development until abruptly terminated by erosion channels the pinching out is gradual. The disappearance is depositional rather than erosional in its character.

LOGAN.

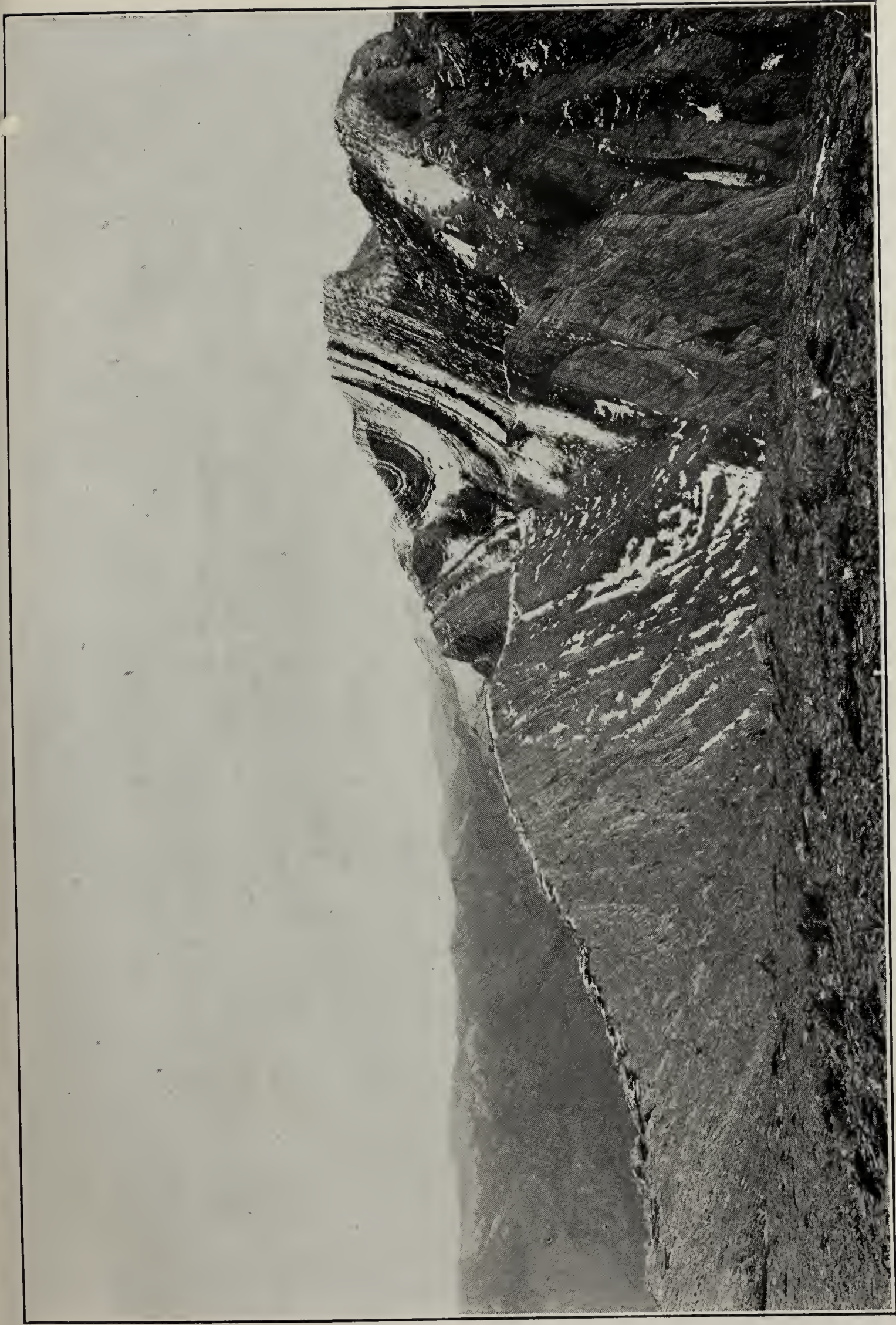
Giant Bay Bar of Ancient Bonneville Lake. When Gilbert wrote his sumptuous history of ancient Lake Bonneville many of the most interesting and instructive features were quite inaccessible

to the ordinary traveler. Now all is changed and the inconveniences of that day are entirely removed. Recent construction of a railroad throughout the length of the old lake bottom obviates all the insufferable terrors of desert travel. The opening up of a new main line of the Los Angeles and Salt Lake road brings to hand numberless fine lake features. This main line traverses the Toetle Valley, on the west side of the Oquirrh Range of mountains. As the grade cuts into the north end of this range above the shore of the present Great Salt Lake a score or more of the best preserved ancient beaches on the mountain side are plainly visible from the car window.

In ascending the Toetle Valley thirty miles the road grade rises 1000 feet, and deeply excavates the highest Bonneville beach at the pass into Rush Valley. The pass is blocked by the great Stockton gravel bar, which as it formed, made of the broad bay beyond a separate lake of no inconsiderable dimensions. As when Gilbert mapped the region a long spit extends to the southward and around a spur of the mountains and a gigantic bay bar stretches out at almost right angles to the spit and reaches westward to a spur of the Aquia Range on the opposite side of the valley. For many miles the road-grade follows closely a long line of sea-cliffs which sharply mark the highest stage of the old lake. At the pass the grade-cutting across the bar follows around the margin of the spit for a distance of more than a mile.

The bay bar is a huge embankment rising 400 to 500 feet above the valley floor on either side, and nearly two miles long. It is composed of small subequal boulders or large gravel stones. The exposures in the gravel pits which the railroad has opened up for fillings are 50 feet high and quite uniform in texture. The broad flat top marks the old level of the lake waters. On the sloping sides of the great bar are numerous minor beaches denoting brief pauses as the lake waters were receding and lowering.

For a thousand miles the railroad traverses the Great Basin. It covers nearly every phase of Gilbert's work. Along the mountains where faults should be displayed, according to his Basin Range theory, deep road-cuttings reveal none. Alleged fault-scarps are plainly normal mountain girdling due largely to sand-blast action, a process of desert erosion unrecognized in Gilbert's time. Many of the so-called sea-cliffs of the Salt Lake desert are found to be not such at all, but effects of undercutting by



FLEXING OF CANADIAN FRONT RANGES

impinging sands at the line where surface of intermontane plain meets steep mountain side. In the long distances, the railroad traverses lowland plain which on careful inspection is discovered not to be miles deep with mountain débris, as Gilbert claimed, but to be worn out on the basset edges of upturned strata, with soil only a few inches thick. A special desert geology was unknown at the time the Lake monograph was written.

KEYES.

Thrust at Crow's Nest. In the Front Ranges round about the Glacial National Park, Montana, evidences of reverse faulting are many. The development of thick, stiff and unyielding limestone plates followed by great thicknesses of plastic shales enables identical compressive forces to express their activities very differently from what they do farther south.

Where, in Colorado, the Rocky Mountains are bulged up into huge but simple arches, and in New Mexico into multiplex but open folds, in Montana and Alberta somewhat more intensified movement or more concentrated power causes the unsymmetrical fold to rupture and glide over itself. The transformation of folding into thrusting appears to be due only partly to increased compressive activity. It seems partly the result of the character and succession of the rock terranes.

North of Glacial National Park, where the southern line of the Canadian Pacific Railroad crosses the Rockies at the low Crow's Nest Pass, overthrust phenomena are displayed with such wonderful clearness that the structures are satisfactorily viewed from a distance of many miles. East of the Pass Crow's Nest Mountain rises out of the plains as a gigantic flat-topped butte. Lithologically the substructure of the surrounding plains and of the pedicel of the mountain itself is composed of soft shales of great thickness. Hard limestone forms the walled capping of the butte. It is Carbonic in age; while the shales are of Cretacic date. The plane of juncture of the two is sensibly inclined, and crumpled, and the upper surface of the shales below shows all signs of severe over-riding.

Superposition of the older Carbonic limestone over younger Cretacic beds might prove puzzling were it not for the fact that in the main mountains to the west the same phenomena are also finely displayed over a broad field. The distance of over-riding

is probably in excess of 25 miles. The existence of such extensive overthrust is rather unexpected along a mountain front that has always been believed to be one limb of a great crustal plait.

The chief significance of the overthrust at Crow's Nest is not that it is anything novel or rare. It does give, however, an idea of what to expect in other parts of the Cordillera, even where such effect is little suspected. Heretofore Rocky Mountain faulting has been entirely limed on the normal-slip basis. That some of these faults should prove to be thrusts is most illuminating. That others will doubtless turn out to belong to the same category is now to be expected. Even some of the great faults of the Grand Canyon region and the High Plateaus now need careful examination anew in order to determine whether on this basis some of the long inexplicable features recorded do not find more satisfactory explanation. The same is true of the desert ranges of the Great Basin and of the Mexican tableland.

In so many respects recently have our notions concerning the nature of western tectonics undergone fundamental change that in regard to the dislocative features we may have soon to add another revolution of ideas. KEYES.

Biplanation of Earth's Straticulate Crust. Always contraposed to the facial wrinkling of our planet because of orographic trouble is a smoothing out of the selfsame inequalities of relief by agencies which we designate erosive. Whether the planing off process be accomplished by the waves of the sea, which we distinguish as marine denudation, by regional corrasion of streams, which is known as base-leveling, or by provincial wind-scour, which is termed deflation, a flattened surface is the final effect and the ultimate relief expression is a plane lying close to sea-level.

Peneplanation is the leveling off of the upper, or outer, surface of the earth's crust. It is the smoothing out process which alone is usually considered. There is, however, another planation of the earth's crust of which little or no notice ever has been taken. It is that of the lower surface of the straticulate layer. Epicene powers are not involved. Yet the truncation of the folded strata and stratified structures is probably as real and as extensive as the other with which we are more familiar. Passage from the zone of rock-fracture to that of rock-flowage, as characterized by schistose structures, is the ocular demonstration of the existence of the process.

In an area undergoing extensive sedimentation and concomitant down-sinking the bottom of the column of the older sediments may reach depths that are beyond the limits of fracture. The extralimital portion flows off and, undergoing recrystallization of constituent components, reappears as schists from which all evidences of sedimentary origin may have disappeared. Although perhaps not so sharply defined, the level at which the change takes place must be essentially a plane. So that the folded straticulate crust is really bevelled above and below. It may be that many of the low-angled thrust-planes which we always regard as expressions of compressive movement are really phenomena of under-crustal planation.

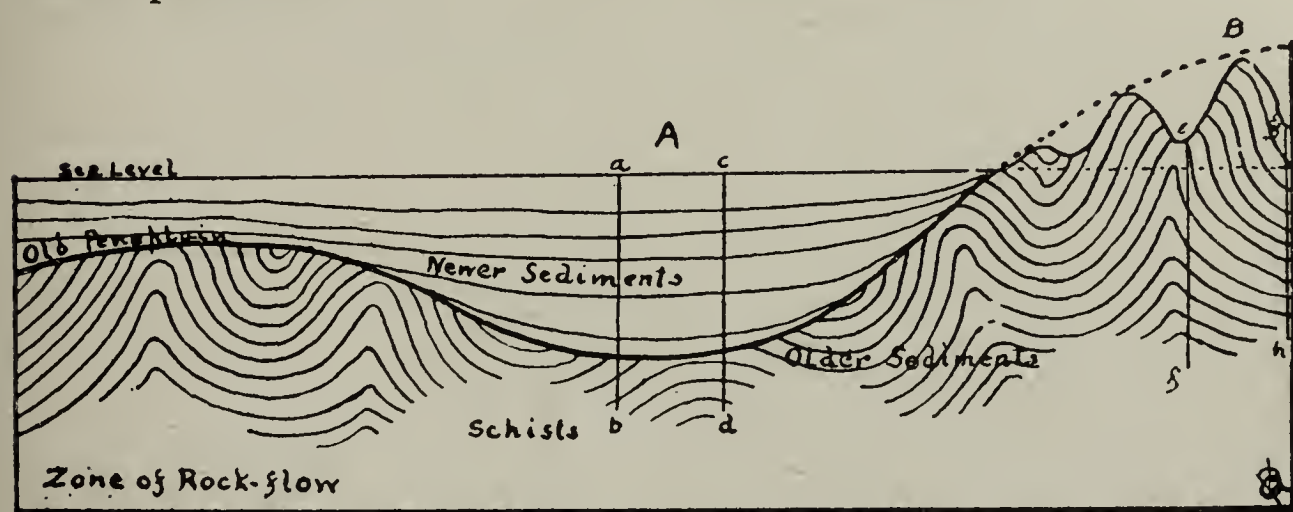


Fig. 11. Geological Directrix of Isostasy

The expression of the phenomenon in diagram is represented in the annexed cut (fig. 11). The figure also indicates the character of the mobile datum plane from which geology estimates the intensity of isostasy.

KEYES.

Flexures in Canadian Front Ranges of Rockies. Notably close folding in the northern section of the Rocky Mountains presents strong tectonic contrast to the open flexing characterizing the southern part of the Cordillera. The extremely appressed features displayed on the west side of the mountain axis, where the pre-Cambrian strata measure 30,000 feet in thickness, suggests possible fan-structures which are usually associated only with the Alps, made classic by Heim and other of the Swiss geologists.

But it seems wholly unnecessary to travel to far-off Switzerland in order to view typical Alpine structures in mountain-building. Jasper Park, in northwest Alberta, appears to afford as fine displays of this description as any of those of which we know abroad.

The Canadian region was made readily accessible by the construction of two transcontinental lines of railroad. Canyons along the Frasier River and the Miette River, the chief headwater of the Athabasca River, present unparalleled sections across the Cordillera in walls 2000 feet high. This superb natural cross-section is supplemented again and again by other exposures showing details in endless profusion and variety.

The Cordillera in Jasper Park, as in other parts of the mighty mountain chain, is characterized by great thrust-planes, but unlike in other parts of the uplift, there is also sharp flexing on a large scale (plate ix, from photograph by D. B. Dowling). The especially notable feature is the Appalachian, or Alpine, structure; and the relationships of the various rock members are presented with far greater perspicacity than possibly anywhere else throughout the

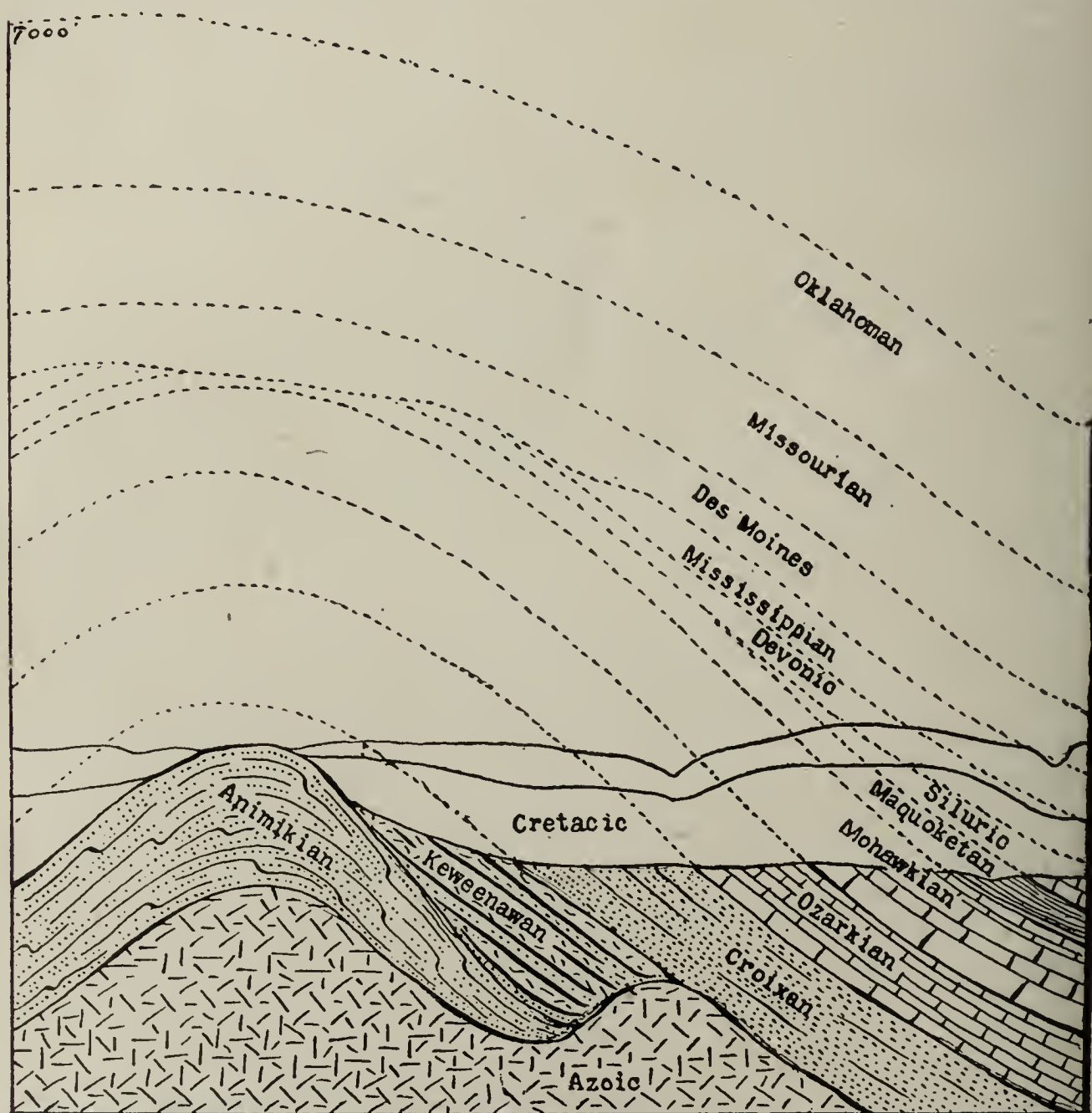
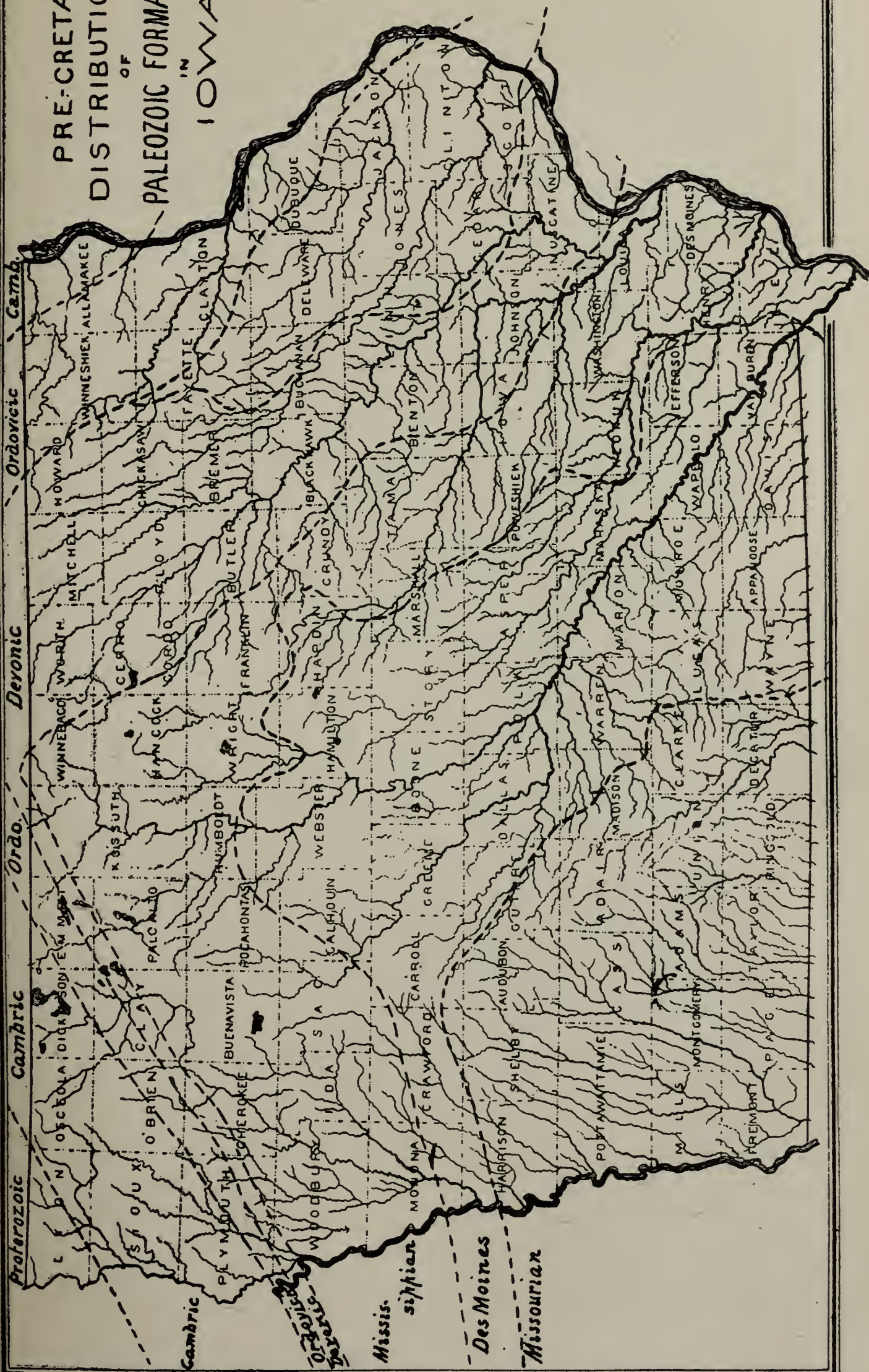


Fig. 12. Magnitude of Siouan Fold

PRE-CRETACIC
DISTRIBUTION
OF
PALEOZOIC FORMATIONS
IN
IOWA



entire extent of the Rocky Mountains; perhaps with greater graphic distinctness than Appalachian structure is exhibited anywhere in the whole world.

KEYES.

Some Prairie Tectonics. Soil conditions on the Great Plains give small clue to the geological structures beneath. But there is recently revealed information derived from deep-well records that points to the fact that the singularly flattened and monotonous prairies have not always been so. In northwestern Iowa and southwestern Minnesota there persist traces of once larger relief contrasts.

Despite the facts that plain is now the dominant topographic expression of the entire region, that the eye has unobstructed view in every direction for scores of miles, and that the horizon is as unbroken and as straight as the sky-line of a summer sea, there formerly existed here a high and mighty mountain range. Extent, form and structure of this ancient chain may now be figured forth and its characteristic facial expression pictured out.

This great earth-wrinkle which like Thetis rising from the waves sprang out of Mesozoic seas extended from the eastern shore of present Lake Superior southwestward far into Nebraska. Medially the strata composing this majestic range were bowed up more than a mile above the present level of the prairies. In their prime these Siouan Mountains rivaled in scenic beauty and grandeur the Adirondacks, the southern Appalachians or the Juras of today.

As they appear on the general geological map of Iowa the Paleozoic formations are distributed in relatively narrow belts trending northwest and southeast across the northeastern corner of the state. Very singularly it had always seemed these belts abruptly terminated at the north very soon after passing the state boundary. Far to the north in Manitoba, there is the same narrow belting of the same formations and, as in Iowa, the strike is still northwestward, and in line. The Canadian Paleozoic area is separated in central Minnesota from the Iowa field by a broad tract of pre-Cambrian crystallines.

These pre-Cambrian rocks form the core of a rather notable arch, the axis of which runs northeast and southwest. The exposures of Sioux quartzite constitute the western nose of a canoe-

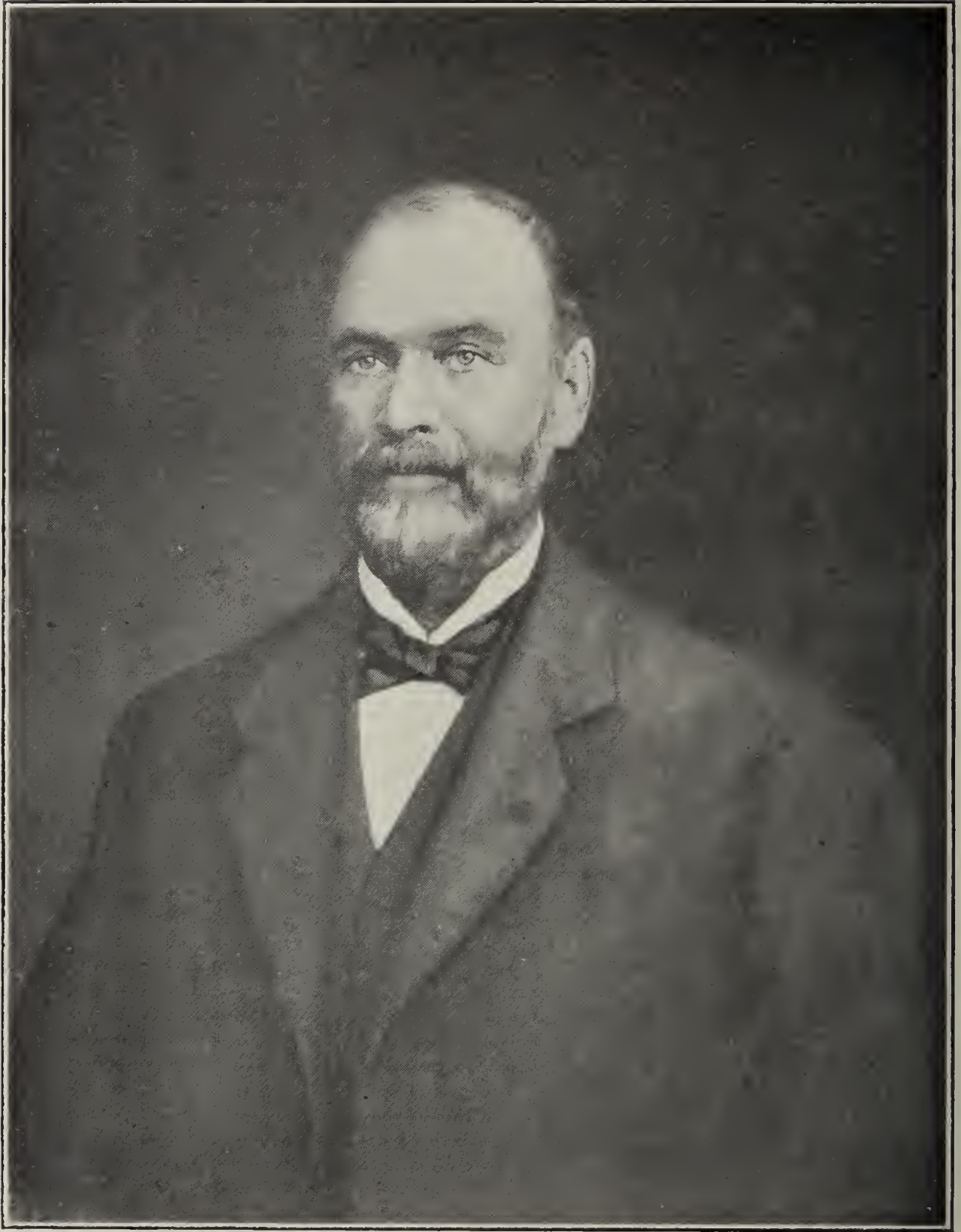
shaped fold as the latter plunges beneath the post-Paleozoic rocks of the Great Plains region.

It is against the south slope of the sharp Siouan anticline that the belted Paleozoic terranes of northeastern Iowa are upturned, and apparently cut off. Bearing in mind the geographic position of the recognized anticline, an arch between the crest of which and the base of the limbs there is a stratigraphic interval of more than 5000 feet, it is obvious that the Paleozoic belts originally did not really terminate against it in southern Minnesota, but rather extended over it, or across it before elevation, and were continuous with the Canadian belts. Such being the case it is equally obvious that the Iowa belts should not terminate against the remnants of the arch, but should swerve sharply and pass westward, in a direction parallel to the axis, but beneath the Cretacic cover. This is found actually to be in accord with recently observed facts. KEYES.

Tectonic Setting of Utah's High Plateaus. To visualize properly the larger structural relationships of the high Plateaus of Utah the latter have to be projected as features of the Colorado Dome of Arizona. Over a vast central tract of this dome now spreads as a surface cover a thick plate of very resistant limestone of Carbonic age, the medial area of which has been swept clean of the great mass of shales and soft sandstones which once mantled it. Around the flanks of the dome these weak, or non-resistant, beds appear in successive belts, their more indurated layers forming the coping of inward facing escarpments, or cliff-lines.

Above the smooth clean limestone rise occasional buttes and plateau plains of limited areas preserved by remnants of ancient lava-flows the substructure of which are the red shales and soft beds which elsewhere reposed unbrokenly upon the limestone throughout the region. Recent volcanic cones also now dot the dome surface. Around the San Francisco Mountains, the chief peak of which rears itself a full mile above the plain, are grouped 400 to 500 smaller volcanic vents and their characteristic ash-cones. But these surmount the dome surface, having come into existence long after the removal of the shale mantle.

Save at the Grand Canyon the great limestone surface plate is untrenched by important drainage ways. KEYES.



CUMMING PORTRAIT OF PROF. SAMUEL CALVIN

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MAJOR FEATURES OF EARTH'S SURFACE ¹

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Many debatable questions concern the permanency of the continental platforms and the ocean basins. These questions assume new turn by the recent hypothesis of Wegener, according to which, instead of the former sinking of great continents into abyssal depths, as accepted by many geologists, the land bridges demanded by the paleogeographers are thought never to have existed, but continental masses are regarded as having split apart and become widely separated because of horizontal creep of the lighter salic continental blocks over the deep-lying heavier Sima.

Through the separation of America from Europe, the Atlantic Ocean arose, and in the same way anterior India and Australia were separated from the African block and shoved toward the northeast. The author then showed that the processes accepted by Wegener lead to striking contradictions with the proved results of paleogeographic investigations, but added that it was necessary to proceed much more carefully in the making of paleogeographic reconstructions than had been done in many cases. The shoving of India northeast across the Indian Ocean is contradicted by the

¹ Translated from the German; with notes and comments (in brackets) by Charles Schuchert. The summary originally appeared in the *Mitteilungen d. k. k. geol. Gesellsch. in Wien*, LVIII Bd., pp. 268-270, and 329-349.

homogeneous and unbroken sequence of Mesozoic shelf deposits on the shore of Tethys from anterior India to Australia. The former close union between Europe and America, postulated by Wegener, because of the general harmony prevailing between the Armorican chains of Belgium and the Appalachians, and through the breaking up of which the Atlantic Ocean originated, need not be thought of as the welding together along a wide front of Fennoscandia and Laurentia and their southern marginal mountain arcs. Such great horizontal movements, furthermore, demand a former wide separation of eastern Asia from Alaska, which is entirely inadmissible because of the connected zone of coastal features around the Pacific Ocean. Just as little can we bring the folding of the Andes into a casual relation with the separation of America from Europe and Africa, since, for example, Central America does not have at all an Andean structure.

Finally, the Permian Ice Age cannot be explained simply by the horizontal pushing about of the continents, since we should then have a North Pole in the vicinity of the present Florida, in which region all glacial evidence is lacking. Thus all the weightiest arguments are against the Wegenerian hypothesis, which is, however, understandable as a reaction against certain paleogeographic speculations. As examples maps of the Triassic continents drawn by Haug, with their great spread of lands, and shown on a map of the same time drawn by the author indicate how the continental masses across the North Atlantic and the South Atlantic and Indian Oceans accepted by Haug and others can be justified paleogeographically. To explain the former connection between Europe and North America, a land-bridge is entirely sufficient in the region of the present Wyville Thomson ridge; the union of South America and Africa, which was assumed because of the spread of the Early Cretacic Uitenhage fauna could have been brought about across an archipelago and Antarctica. And lastly, the character of the Permian and Triassic land forms in Brazil and South Africa testifies against a continental [desert] climate such as would necessarily be demanded by a southern continent of such magnitude. On the other hand, a union of anterior India with South Africa over Madagascar must be accepted on account of the common reptilian fauna of Triassic times. However, the great Pacific continent of Mesozoic time,

which Haug theoretically postulated on the basis of geosynclines bordering the lands of the Pacific, is entirely hypothetical.

There are, moreover, no guiding points for the reconstruction of such great continents where the oceans are now, as postulated by Haug and others. The great oceanic basins were certainly in existence in Triassic times, probably were even indicated in the Cambrian period. Paleogeography must, therefore, in order to attain safe results, start from the present and use only the determined results of research in the reconstruction of maps of the older periods. Then, aside from the changes in the labile Mediterranean zones, the conviction of a very far-reaching permanence of the major forms of the earth's surface will become ever more firmly established, as was held by Willis, and there will be no compelling evidence for an extended creep of salic continental blocks. Furthermore, the progressive breaking up of the lands postulated by Suess loses its exaggerated importance, and it even appears that there was an increase of land through the disappearance of Tethys and the welding of the circum-Pacific geosynclines, compensating for the losses through continental fracturing.

In the discussion which followed the reading of the paper, Professor Brückner emphasized the fact that the tremendous loss of water on the earth made necessary by the assumption of such great continents as Haug postulated must negative this kind of reconstruction. As evidence against absolute permanence, however, stand the bottom soundings which have been taken by Philippi out of the tropical Atlantic, since here under the red deep-sea ooze occur sediments laid down near the land, followed beneath by Globigerina ooze. In this connection, Professor Diener suspected volcanic sands in the so called near-shore sediments; the red deep-sea ooze is, however, according to Philippi, apparently a product of the very cold water of the ice age, which, in consequence of high CO₂ content, might have loosened up the Globigerina ooze, and it might have been formed in about 3000 meters depth. Professor Suess, referring to the Haug geosynclinal hypothesis, was of the opinion that the "Decken" structure of the Alps predicted a very great width of the sea. Professor Diener emphasized the fact that we must differentiate sharply between epicontinental ingression seas, such as was Tethys, and the oceanic basins; further, that geosynclinal areas are by no

means all regions of folding, and it is also wholly hypothetical to assume that geosynclines must be surrounded by continents.

The relief of the earth's crust is dominated by the contrast between the continental platforms and the ocean basins. Continents and ocean basins are the two chief features of the earth's surface. In comparison with them, all other features in the relief of our planet appear as architectonic by-products of secondary importance.

The question of the relative permanency of the ocean basins and land masses is a fundamental one in our science, and since the middle of the last century has been the subject of repeated discussions by geographers, geologists, and geophysicists. It must not be in any way confused with the question whether the relation between land- and sea-surfaces has always remained the same or has changed repeatedly in the course of the earth's history. The problem should be stated as follows: Do the continental masses and the ocean basins lie to-day practically in the same places which they occupied in the Cambrian Period? On practical grounds the question should be thus formulated and not extended to include an agreement of the present continental masses and ocean basins with those elevations and depressions which were formed at the first consolidation of the earth's crust, since in paleogeographic research a reliable basis can be obtained only with the first appearance of fossiliferous sediments, whose exact age can be stratigraphically fixed.

It would be of little value to discuss here in detail the historic development of the question of the relative permanence of the continental abyssal regions. On the one hand, many of the assumptions which served Dana and [especially] Lyell as starting points in the first conflict of their opinions; on the other hand, the excellent presentation of the status of the question given by Penck² in 1894, in his "Morphologie der Erdoberfläche," still holds good to-day in all respects.

As a result of increasing knowledge and the constantly growing emphasis on the difference between the transgressing epicontinental seas and the earth-encircling oceans with their mighty abyssal regions, the question of the permanence of continents and ocean basins has in general been gradually shoved aside in the sense

² *Morphologie der Erdoberfläche*, I, p. 174, 1894.

that it is no longer the former but the latter which are the chief object of discussion. It is interesting to note that Charles Darwin³ already in his time laid the main emphasis on the establishment of a permanence of the ocean basins. The present oceans, he thought, were neither in the Paleozoic nor in the Mesozoic era replaced by land masses or by very wide-spread island groups.

The same point of view appears to-day urged by the leading paleogeographers of North America, Willis,⁴ and, in somewhat lesser degree, Schuchert.⁵ The strongest adherents of the permanence of ocean basins at present are Willis [and Chamberlin]. He calls⁶ the ocean basins, even such abyssal regions as the Gulf of Mexico or the Caribbean sea, permanent features in the relief of the earth's surface," which at least since pre-Cambrian times have lain in the same places as to-day, with only slight changes in their coast regions. In contrast to him, Edward Suess ascribes very different ages to the various oceans. While he recognizes the great antiquity of the Pacific Ocean, he holds the Atlantic and Indian Oceans to be considerably younger. "We must," he thinks⁷ "not only admit the loss of a great Paleozoic, Mesozoic, and Tertiary ocean in southwestern Eurasia, but also great subsequent changes in the middle and southern Atlantic: Geologic facts do not establish the permanence of the great deeps, at least in oceans of the Atlantic type, indeed do not even make it probable." The profound influence of the teaching of Suess upon the development of Geology resulted in almost all European geologists and geographers becoming opponents of the theory of the permanence of continents and ocean basins.

The admissibility of the conception of a complete interchange of sea-basins and continents in the sense of Lyell, has met with strong opposition in the last decade, due to geophysical research. The maintenance of an equilibrium, an isostasy, between the continental masses and the oceanic basins leads to the recognition of the fact that the former are composed of rocks of less density

³ Origin of Species, German trans., p. 458, 1872.

⁴ Science, N. S., Vol. XXXI, No. 790, 1910.

⁵ Bull. Geol. Soc. America, Vol. XX, 1910.

⁶ Journal of Geology, Vol. XVII, p. 203, 1909; also W. D. Matthew, Climate and Evolution, Ann. New York Acad. Sci., Vol. XXIV, pp. 174, 179, 305, 308, 1915.

⁷ Are Great Ocean Depths Permanent? Natural Science, Vol. II, No. 13, p. 186, 1893. The incorrectness of the comparison of Tethys with the modern oceans has been pointed out by Penck (l. c., p. 184).

than the ocean bottoms. The former belong predominantly to the region of Sal, the others to that of Sima, as defined by Suess. Since the limit of extent of Sal and Sima on the earth's surface seems to correspond essentially with that of the continental masses and the ocean basins, this fact would warrant a relative permanence of the two major forms of the crust.

In striking contrast to these results of geophysical researches stand certain paleogeographic investigations, since they seem in many cases to require, between continents, land connections which later have sunk into the abyssal depths of the world-sea. These land bridges have been postulated by many paleogeographers to such an areal extent for the Paleozoic and Mesozoic eras that the present relationship of land to water over the earth's surface has been changed completely around. Especially is this true in the reconstruction of the distribution of land and sea during Mesozoic times, in which almost all paleogeographic works show a gigantic land-mass in the southern hemisphere, extending from the east coast of South America over Africa and the East Indies to the west coast of Australia.

The hypothesis of the sinking of such extensive masses of salic rocks in the region of Sima is contrary to the results of the gravity measurements on the oceans.⁸ Wegener⁹ has recently undertaken to reconcile the demands of the paleogeographers and the geophysicists by seeking to explain the major features of the earth's surface genetically through the theory of the horizontal movement of the continental masses. For the hypothesis of the sinking of former land connections he substitutes the theory of the splitting off and horizontal shifting of the salic continental masses which, so to speak, swim on the zone of Sima, and whose surfaces in general coincide with the floor of the great world-sea. In this way the Atlantic ocean arose, through the splitting off of America from Europe-Africa, and in like manner anterior India and Australia, together with Antarctica, were cut off from the African mass and the two former lands shifted to the northeast into their present position. The beginning of the

⁸ The late Prof. Joseph Barrell left an unpublished paper on this subject, showing how a lighter crust can be loaded sufficiently to sink it into oceanic depths; see "The Evolution of the Earth and its Inhabitants," p. 43, 1918.

⁹ Die Entstehung der Kontinente: Petermanns Geog. Mitt., I Bd., pp. 185, 195, 253-256, 306-309, Gotha, 1912; also, Geol. Rundschau, III, pp. 276-292, 1912.

Atlantic, according to Wegener, corresponds to an almost universal crowding of the continents toward the Pacific Ocean. On the coasts of the former pulling and splitting off are the rule, on the coasts of the other, pushing together.

Wegener's explanation of the major forms of the earth's surface demand all the more consideration since it destroys the fixed basis of our former method of paleogeographic reconstruction of continents and seas. Dacque,¹⁰ one of the representatives of modern paleogeography, has agreed with Wegener, as has also Klöcking,¹¹ who welcomes his theory as a worthy support of Simroth's law of biological development. Andree¹² has expressed doubts of the formation of the Atlantic Ocean by the splitting off of North America from Europe, without, however, weakening Wegener's hypothesis by the presentation of proofs.

If we test Wegener's idea of the separation and recombination of the continental masses in the light of the events in the earth's history, we arrive everywhere at striking contradictions with established results of paleogeographic research.

Let us begin with the three continents of the southern hemisphere. According to Wegener, they were all long ago united with the African block, which later was left alone in its original place. South America was connected with Africa on a broad front and its splitting off in Eocene or Oligocene times was the cause of the origin of the South Atlantic.¹³ The southern point of anterior India lay near that of South Africa. In Triassic or Jurassic time, the Indian peninsula was split off from the African mass, wandered northward, and finally during the later Tertiary period was united with Eurasia. The Australian continental mass was still united with the Indo-African block in Permian time, and Wegener points to the probability of a direct connection of the west coast of Australia with the east coast of anterior India, while the other side was connected along its whole southern coast with the Antarctic mass. The latter was shoved later toward the

¹⁰ Grundlagen und Methoden der Palaeogeographie, p. 182, Jena, 1915.

¹¹ Simroths Entwicklungsgesetze im Lichte der Wegenerschen Hypothese, etc.: Petermanns Geog. Mitt., I Bd., p. 121, 1913.

¹² In his book: "Ueber die Bedingungen der Gebirgsbildung," Bornträger, Berlin, 1914, Andree objects only to the association of the shifting of the continental masses with the erection of folded mountains on their edges.

[¹³ If this splitting off is admitted, it must have begun at least as early as Early Cretacic time, since marine strata of this age are present in northeastern Brazil.]

Pacific Ocean, but maintained its connection with Australia until the Quaternary times; while the union of Australia with anterior India must have been broken through at least during Jurassic times.

The folding of the South American Andes and the Himalayas was brought by Wegener into genetic relationship with the breaking through of the South Atlantic and with the joining of the peninsula of anterior India with Eurasia.¹⁴ The shelf which formed the free shore of the separated continental mass, with its thick sediments, was pushed up in folded mountains. If we accept this premise of Wegener's, then we arrive at the conclusion that the Triassic and Jurassic sediments of the Himalayas and the Salt Range must have been deposited, not where they are to-day, but between the Equator and the Tropic of Capricorn, and that the mediterranean zone of Tethys started in the Indian Ocean. The latter, under this hypothesis, must, during the whole Mesozoic era, have covered that area which now is occupied by the East Indian masses and which at that time still lay alongside of South Africa.

The radical differences between the vertebrate faunas negative with great finality a union of South America with Africa lasting into Middle Tertiary times.¹⁵ Not only had South America built up a special development center for the mammals, isolated from the neighboring regions, from the earliest Eocene on, but this differentiation among the older vertebrates reached back at least to Permian times. If South America were really connected along its east coast directly with the west coast of Africa, how can we explain the complete absence in South America of the rich Permian and Triassic reptilian and Stegocephalian fauna of South Africa? In both realms the same life conditions existed during the Anthracolithic period, and during the Late Carbonic Period the *Lepidodendron* flora of the northern hemisphere spread on the one side so far as Tete on the Zambesi River, and on the other side to Rio Grande do Sul, in Brazil, and was later succeeded by the *Glossopteris* flora, which already in the Permian times had

[14 The Andes, however, appear to have been folded at the close of Cretacic times, and their present elevation is due to vertical uplift in Pleistocene time. These facts do not fit in with Wegener's time-origin of the South Atlantic.]

[15 Most American paleontologists have given up all connections between these lands at any time in the Cenozoic era.]

pressed on so far as the Dwina River in northern Russia ; and yet on the South American continent every equivalent of the South African fauna is lacking. The meager relations which are indicated by the occurrence in South Africa and in Brazil of Proganosauria that had a marine ancestry, point to a loose connection between both continents over an archipelago and the Antarctica, rather than to the existence of a firm, broad land-bridge.

The separation of the Indo-Madagascar portion of greater Gondwanaland from the African block must have followed later in Liassic times, since those striking relations between Ethiopian and Andean marine faunas, which later reached their maximum in the spread of the Early Cretacic Uitenhage fauna from Cutch to Malone in western Texas, and which had as a necessary concomitant the opening up of the Strait of Mozambique, were initiated as early as the deposition of the Upper Liassic sediments of Malvatana. This separation can therefore also explain¹⁶ the invasion of the Triassic land vertebrates of Eurasia into anterior India by way of Africa, but not the incursion of *Megalosaurus* and *Titanosaurus* into anterior India and Madagascar. It must also have been in the time between the Late Liassic and the Late Jurassic times that the Indian continental masses made their way from the Strait of Mozambique to a position very near to the one they occupy to-day. Then the invasion of anterior India and also of Australia by *Megalosaurus* could no longer have taken place over the already opened Strait of Mozambique, but only from the northwest over one of the island bridges of Tethys, which we conceive of, as did Penck, not as an ocean, but as a mediterranean sea swarming with archipelagoes and indented with bays, whose single basin could only have been formed gradually through small sounds, like the present Hellespont or the Bosphorus.

On the other side, however, Madagascar could not yet have been separated from India itself in Late Cretacic times, since on this island, also, are found remains of those great land reptiles. If we accept a separation of the shores of Madagascar and India through a breaking off of the latter, according to Wegener, then no invasion of Madagascar by the Cretacic dinosaurs of India

¹⁶ Bearing in mind Handlirsch's justifiable criticism of the landbridge theory, I shall limit myself here to the great land animals, whose passive spread over wide sea-ways is impossible.

could have taken place, nor one of India from the north shore of Tethys over the Indian Ocean. These considerations force us to return to the old premise of the union of anterior India with Madagascar by a land-bridge, which later sank beneath the surface of the Indian Ocean. We must, then, be willing to accept the fact that Madagascar was separated from India in post-Cretacic time, and that it wandered back over the same route which it formerly took, together with India, only now in the opposite direction, toward the Strait of Mozambique, a wandering back and forth of continental masses which no longer corresponds to a pressure from all sides toward the Indian Ocean.

A land union must have existed also between the East Indies and Australia in Early Mesozoic times, as proved by the occurrence of *Megalosaurus* in the Late Jurassic sandstones near Cape Patterson, in Victoria.

The sediments which to-day are piled up in the Himalayas to heights of more than 6,000 m. must, according to Wegener, have been laid down in the shelf seas of the continental mass of anterior India. This premise is not too far-fetched, since, for example, in the marine Triassic beds of the Salt Range remains of *Gonioglyphus longirostris*, one of the Stegocephalia known only from the Gondwana formation of India, have been found. But since the Indian mass was still connected during Triassic times with the African block and Madagascar, this assumption is however untenable, in view of the South African faunal elements in the Late Triassic land fauna of anterior India — and since India must have wandered during the Liassic and Jurassic periods at least 44 degrees to the north of its original position, we should expect to find very clear evidences of diastrophic movements in the sediments of the bordering shelf region. Such evidences are utterly lacking.

According to the unanimous testimony of all observers, the series of marine sediments in the Himalaya region shows only a single gap, that at the base of the Permian Kuling beds. Over against the idea of the uninterrupted series of beds, we might place the theory that sedimentation went on undisturbed by the relatively slow movement of the Indian continental mass through the Indian Ocean, and that the changes in the faunas came about gradually. That such an assumption is not sound, will be demonstrated presently.

The problem of a pushing of the Indian mass to the north brings in an insurmountable complication, due to the fact that far to the east of the peninsula of anterior India a remnant of Gondwanaland appears in the *massif* of Camboda. Geologic structure, sedimentary sequence, Mesozoic land fauna and flora leave no doubt concerning the close relationship between the *massif* of Camboda and anterior India. Between them, however, is the system of the Burman folded arc, made up of a thick series of marine sediments. Whoever accepts Wegener's hypothesis must agree that in this case we are dealing with sediments of an epicontinental sea that transgressing over a continental mass, which on one side in the Indian peninsula, and on the other in the Camboda *massif*, towered above the surface of the sea, and which underwent jointly the horizontal movement to the north. To the shelf region of this continental mass must, however, have belonged the Sunda Islands, the agreement of their Mesozoic sediments with those of the Himalayas being such a far-reaching one that both must be considered as formations of a single deposition region. This agreement is especially true for the Jurassic sedimentation, which shows precisely similar faunal and lithological characters on the Moluccas and in Spiti.

The Mesozoic formations of this Himalayan sedimentary province extend, however, far into the interior of Tibet and southern China, that is, into a region which belongs to the Eurasian shelf zone. In the formation of the mountains within the mediterranean zone of Tethys, considerable portions of the coastal shelf of Eurasia, as well as Gondwanaland, obviously take part. However, it can not equally well be said that in the welding of the Indian mass on to the Eurasian, only the shelf of the former was pushed up into folded mountains. The tectonic continuation of the central Asiatic chains of the Himalayas as far as Tien-Schan in anterior Asia shows that this can not be the case. And now the question naturally arises, where does the boundary lie between the sediments of the Eurasian shelf and of Gondwanaland? Is there such a thing? Both questions must be answered in the negative on the basis of our stratigraphic observations.

For those who share the view that Gondwanaland has always been in the same place in which it now lies, it is obvious that both questions are without any weight. In a single deposition

province, such as the east half of Tethys represents, we need not expect differences in the sediments on the north and south coasts. We must, however, expect these, if we remove Gondwanaland 44 degrees to the south, out of the realm of Tethys, and especially during that time when the Indian mass was broken away from its union with the African block and surrounded on all sides by the abyssal depths of the Indian Ocean, which opposed impassible barriers to any addition of neritic faunal elements from Tethys. A very strong objection to Wegener's hypothesis therefore exists in the similarity between the united sedimentary zones of Gondwanaland and Eurasia occurring in the mountain chains of the Himalayas, of farther India, and of southern China.

Since Wegener brings the folding of the Himalayas into a casual relationship with the uniting of the Indian mass to Eurasia, he must of necessity ascribe this union to the earliest possible division of Tertiary time, since the principal folding of the Himalayas falls in the epoch of the deposition of the Siwalik beds. It has been shown above that that union must have been existent in the time of the Late Jurassic (*cf.* the spread of *Megalosaurus* to Victoria). Tertiary land vertebrates, which are apparently older than the Siwalik fauna (Manchhar and Bugti fauna), point likewise to the faunal exchange between anterior India and Europe during a period long previous to the principal foldings of the Himalayas, and make the association of the latter with the horizontal movement of a continental platform completely untenable.

Since Australia must on the one side have been torn loose from the Indian mass at the beginning of the Cenozoic era, and on the other side remained united with Antarctica until the Quaternary era, the single Antarctic land connection which was at all likely for Tertiary time, *i.e.*, that between South America and Australia, seems to be untenable for the followers of the Wegener hypothesis.

Wegener sees a main argument in support of the supposed coming together of the southern continents into a region centering around the south point of Africa, in the thereby assured possibility of explaining the Permian Ice Age. "We only need to place the south pole in the now very much limited realm of ice in order to remove from this occurrence all that is inexplicable." This optimistic assumption I cannot share, in view of the ap-

pearance of definite traces of a Permian Ice Age in the Katanga region (Belgian Congo) reported by Stutzer,¹⁷ and in Togo, by Kört.¹⁸ If we place the south pole in the vicinity of Natal, then we get for Togo on the one side and the region of the east Australian glaciation on the other almost equal distances; both come to lie at 50 degrees S. Then the north pole apparently falls between Florida and the Bermudas, if we follow Wegener's belief that North America in Permian time was united with Europe and western Africa along the west shore of the North American continental mass. Marked traces of the Permian Ice Age are nevertheless found nowhere in the unbroken anthracolithic formations of North America.¹⁹

If we turn to the northern hemisphere, we find a no less insurmountable difficulty to Wegener's proposed origin of the North Atlantic Ocean through the separation of North America from Europe and eastern Africa.

The acceptance of a land connection, repeatedly broken down and rebuilt, between North America and Europe, or, if we prefer the terms made familiar by the work of Suess for the old continental nuclei, Laurentia and Fennoscandia, is impossible on zoo-geographic grounds. The exchange of land faunas between the Old World and the New World begins in Carbonic times and lasts until at least Oligocene deposition. From Miocene on, it could have been carried on over eastern Asia just as well as over Europe. The long-enduring, long-drawn-out interruptions of this faunal exchange are no counterargument against Wegener's hypothesis, since they could also have been caused through transgressions of a shallow epicontinental sea.

As a positive proof in favor of this assumption, Wegener brings forth the parallelism of the two coastal lines of the Atlantic and the further structural relations discovered by Bertrand²⁰ between

¹⁷ Über Dwyka-Konglomerat im Lande Katanga, Belgisch-Kongo: Zeits. d. deut. geol. Gesell., LXIII Bd., p. 626, 1911.

¹⁸ Das Eisenerzlager von Banjeli in Togo: Mitt. aus d. deut. Schutzgebieten 1906, Nr. 19, p. 106; also, Über Goldvorkommen in Togo: Ibid., p. 57, 1910; also, Ergebnisse der neueren geologischen Forschung in den deutsch-afrikanischen Schutzgebieten: Beit. z. geol. Erforschung d. deut. Schutzgebieten, Heft I, p. 17, 1913; also, Gagel, Die neueren Fortschritte in der geologischen Erforschung und die bergbauliche Erschliessung der deutschen Kolonien: Geol. Rundschau, I Bd., p. 203, 1910.

[¹⁹ The nearest tillites are those of Boston, Mass.; these are local ones and may be older than Permian. The next nearest tillites are those of Brazil.]

²⁰ La chaîne des Alpes et la formation du continent Européen: Bull. Soc. Géol. de France, 3d ser., T. XV, p. 423, 1887.

the Armorican chains of Europe and the Appalachians. But the similar structure of Laurentia and Fennoscandia, and the Carbonic folding of the south shore, in no way prove the former direct union. Along with Suess,²¹ we must always conceive of northern Europe and northern North America as fragments of a higher tectonic unity, and need not conclude from such a conception that there exists a direct union of both parts on a broad front. The connection could as well have been accomplished over a comparatively small bridge far to the north of the supposed connection between the Appalachians and the Armoric arc. It is not necessary to postulate a Carbonic folding over the whole extent from the banks of Newfoundland to the breaking up of the continental mass in the west of Ireland, in order that thereby the uniformity of the foldings in Newfoundland and in Armorica should seem not to be interrupted. Interruptions of folded zones over a wide stretch are much more the rule than the exception. Does there perhaps exist a true direct union between the European and the Asiatic Altaides in the sense of Suess? Even so, what right have we to predicate, just such a union between the European and the Transatlantic Altaides?

To this by no means absolutely positive proof of Wegener's, several strong exceptions may be taken.

Every conception of an original union of North America with Europe before the origin of an Atlantic gap demands a marked separation of the North American continental mass from eastern Asia by a deep-sea realm reaching even into the region of Sima. Such a sea-basin, extending over at least 35 degrees of longitude, must have been an impassible barrier to all the benthonic faunas of the neritic and even the bathyal regions. Now, however, individual marine faunas of eastern Asia stand in the very closest relations to those of western part of America. This holds true even for the Cambrian faunas of China and western North America. The Early Triassic *Meekoceras* faunas of the region of Vladivostok and California contain a series of identical, or very nearly related types, and are very different from the contemporaneous faunas of the Arctic and Mediterranean regions. The peopling of the California sea could only have come from eastern Asia. The eventual acceptance of a migration of this fauna from

²¹ Das Antlitz der Erde, III Bd., pp. 59, 89, 1909.

the coast regions of anterior India, which were at that time united to South Africa, around Australia, Antarctica, South Africa, and South America, to California, would be unnatural, in view of the complete absence of marine Early and Mid Triassic beds in all the region mentioned. A no less strong argument against the separation of Alaska from eastern Asia by a deep sea lies in the distribution of the Late Triassic *Pseudomonotis* fauna, which has everywhere been imbedded in sediments of a very shallow sea. The circum-Pacific geosyncline of Haug, which lasted throughout Jurassic and Early Cretacic times, has as an unavoidable corollary an outer continental zone, in no place broken through by a deep sinking.

If we postulate a union between North America and the European continental block across western Ireland, lasting until Late Tertiary time, we must therefore assume a breaking up of the connection between Alaska and the eastern Asiatic peninsula, and the creation of an opening 35 degrees in width in the region of the Behring Sea. Now Alaska, as Suess has shown, and as is coming out still more clearly from the newer work of competent American geologists, is built up precisely after the pattern of the Asiatic island chain. It belongs, in structure, decidedly to eastern Asia, and forms a unit with the latter, at least in the same measure as Laurentia does with Fennoscandia. The unity is self-evident, not only in the contemporaneous and similar mountain-making events, which we may place in the period after the separation of Europe from North America, but also in the similar distribution and appearance of the older sediments beginning with the Devonian, so that a tearing apart of Alaska from the eastern Asiatic peninsula means the breaking up of a natural unit.

Furthermore, Wegener's fundamental postulate of the splitting off of the two American continental masses from that of the Old World with the origin of the Cordilleras, is contrary to the structure of Central America and the West Indies. Costa Rica and Panama are also fragments of a continental mass and do not show the Andean structure at all. Why, we may reasonably ask, is the folded coastal shelf lacking here, and why do the Andes bend sharply away from the western edge of the continental mass and proceed through northern Colombia and Venezuela eastward

toward Trinidad? Would it not be remarkable if the North and South American continental blocks, which at different times are said to have been cut off from those of the Old World, should, after long wandering westward, finally come into contact with each other in a quite restricted zone?

Wegener's hypothesis, alluring as it may seem at first sight, because it appears to bring us nearer the solution of several different problems under a single viewpoint, is nevertheless only a juggling with mere possibilities. There is lacking in it the foundation of positive evidence, and a series of proved paleogeographic results cannot be brought into harmony with it. But it is understandable as a reaction against a direction which paleogeography in Europe took in contrast to that in America, and which went too far in the reconstruction of the former continents.

If we wish properly to judge the permanency question, it will above all be necessary to establish some sort of clarity concerning it, as to how far we may explain determined facts in the realm of paleogeography by the acceptance of bridges over what are now sea-basins. As the best example for this purpose, let us take that period which Dacqué very recently called the most geocratic epoch of the earth's history (*l.c.*, p. 160), that is, that period in which the land exceeded the sea in extent as at no other time. I can not, of course, give here in detail the proof of my results, which are given elsewhere.²²

The distribution of sea and land in this Triassic Period teaches us that the present continents existed almost as such, that only on their edges were they periodically flooded by transgressive seas, and that in both hemispheres the continents were separated by a mediterranean sea, which in no way can be compared with the ocean, but only with the Mediterranean Sea of to-day. Aside from these comparatively narrow labile middle-seas of the lithosphere, the continental masses existed in Triassic times almost as they are to-day. The world-wide transgressions of the Late Jurassic or the Late Cretacic seas found their expression in temporary floodings of the continental masses by shallow waters, without thereby altering the existence of the former as continental masses. All sediments of the transgressing seas, outside of the

²² This evidence is presented in a special contribution, "Die marinen Reiche der Triasperiode," in the Denkschriften der k. k. Akademie der Wissenschaften, 92 Bd., pp. 405-549, 1915.

above mentioned labile, geosynclinal zones, are neritic, [*i.e.*, are shallow-water deposits], but even within the realm of the mediterranean itself, neritic deposits predominate far over all others, those of bathyal character decrease, and abyssal ones, if present at all, are exceedingly rare.

A glance at the paleogeographic maps of Lapparent, Haug, Frech, Kossmat, and others, show us that the difference in the distribution of seas and lands between the Triassic Period and the Present time lies not so much in the realms of the continental masses of to-day as in the sea-basins. All these maps show a united continent in the place of the North Atlantic, which joins Laurentia with Fennoscandia, and a still greater south, or equatorial, continent, which comprises South America, Africa, anterior India, and Australia, and includes also the realms of the southern Atlantic and Indian Oceans. In the restriction of the Indian Ocean, some of the above named paleogeographers go not so far in the case of the Atlantic Ocean. Steinmann and Pompeckj have objected to the connection between the Californian sea and Tethys in the region of the West Indies.

If we consider the paleogeographic facts which would testify such a far-reaching suppression of the Atlantic and Indian Oceans, it would almost appear as if the knowledge of the recent age of the coasts of this ocean had made it possible to regard the formation of the ocean-basin itself as occurring at an equally late date. We may, however, always admit, with Suess, that the circumference of the Pacific Ocean bears the traces of greater geologic age than does the configuration of the Atlantic coast, and yet no assistance is thereby offered in the determination of the age of the oceanic basins.

For the region of the North Atlantic, the acceptance of a land-bridge is indispensable on zoogeographic grounds; only thus could the exchange of land faunas between Europe and North America since the Carbonic times have taken place from one side to the other, and along their coasts; the marine faunas have migrated from one continent to the other ever since Cambrian time. The close relations existing between the Andean marine faunas and those of the Mediterranean realm, during the whole Mesozoic era, demand the acceptance of such a land-bridge, which undoubtedly may have been at times interrupted, and even broken up into

islands, just as imperatively as does the immigration of the reptile fauna of the Keuper beds into the Late Triassic red beds of North America.

Traces of such a land-bridge are present in that ridge [Wyville-Thomson ridge] which separates the Skandik of De Geer ²³ from the North Atlantic proper. This ridge is depressed below 500 m. only in three small channels between the masses of the Shetland Islands, the Faröes and Iceland, and in the Denmark straits.²⁴ The existence of a land-bridge until Mid Tertiary time on the site of this ridge would serve to explain all zoogeographic relations between North America and Europe and between their bordering seas. For the acceptance of a North Atlantic continent, in other words, of a union of Laurentia with Fennoscandia along a broad front, every convincing argument is lacking.

As for the acceptance of an equatorial, or southern, continent, which in the first place was constructed to explain the spread of the Permian *Glossopteris* flora, I have already emphasized the fact that the difference of the land vertebrate faunas of Mesozoic time testifies against rather than for a union at that time of South America with Africa. The only argument that can be brought forth in favor of a union of both continents on a broad front is the spread of the Early Cretaceous Uitenhage fauna of Cutch, India, southward along the coasts of the Strait of Mozambique [and across the South Atlantic] into the region of the Argentine Cordillera and as far north as Malone, Texas. The spread of this littoral fauna, characterized especially by peculiar groups of the bivalve genus *Trigonia*, could have taken place only along the coastal border of a continent, or an archipelago. As such, the coast of Antarctica suggests itself, since it approaches very near to South America through the Antarctic Cordillera, and through an archipelago which we may think of as including the Kerguelen, Crozet, and Prince Edward Islands. It gives evidence of a land union which Ortmann ²⁵ also regarded as one of those still possible during Tertiary time.

A very strong argument against the acceptance of a broad continent bridging over the greater part of the South Atlantic

²³ Kontinentale Niveauveränderungen im Norden Europas: Petermanns Geog. Mitt., LVIII Bd., p. 122, 1912.

²⁴ Maximum depth 649 m. to the south of the Faröes.

²⁵ Map accompanying Reports of the Princeton University Expeditions to Patagonia, Vol. IV, Palæontology, pl. xxxix, 1899.

is offered by the character of the Permian and Triassic continental sediments in Brazil and South Africa. They are in no way, as has been often thought, desert deposits; but as Koken²⁶ has shown to the contrary, indicate abundant water, river plains, swampy and sea areas as the environment of their deposition. Nothing suggests the predominance of the desert climate which we should expect in such a mighty continental realm as the majority of the paleogeographers construct for the Triassic area of the southern hemisphere.²⁷

A land union of anterior India with South Africa over Madagascar during Triassic times must be accepted on zoogeographic grounds, since the former has been peopled from Europe (*Belodon*, *Hyperodapedon*, *Thecodontosaurus*, Labyrinthodonts of very close affinities with *Metopias* and *Capitosaurus*), as well as from Africa (*Massospondylus*, *Dicynodon*, *Bothriceps*). The union with Madagascar, in view of the appearance of *Megalosaurus* and *Titanosaurus*, must have lasted until Cretacic time, when the connection with South Africa was interrupted by the Strait of Mozambique, which in all probability was opened during the Liassic epoch. Anterior India and Madagascar continued from Liassic into Cretacic time as a long narrow island between the Indian Ocean on the east and the western Ethiopian mediterranean of Neumayr, which was a southward extension of Tethys.

Toward the northeast stood the continental platform of anterior India, with the spur of Assam extending farthest into Tethys. At various times during the Triassic Period this continent was united with the *massif* of Camboda — to which belonged the greater part of the island of Borneo — and also with Australia. This connection, which must have lasted with interruptions at least into Late Jurassic times (*Megalosaurus* in Victoria), apparently lay across the Sunda Islands.

All these Triassic land connections which have been established as a result of paleogeographic investigations, decrease, of course, the size of the Indian Ocean but only in so far as they require its separation from the Arabian Sea by a small peninsula, the re-

²⁶ Indisches Perm und permische Eiszeit: Neues Jahrb. für Mineral., etc. Festband 1907, p. 526. Koken in his map marks the South Atlantic continent with a query, and restricts the continent of Gondwana considerably in the region of the Indian Ocean.

²⁷ This exaggerated change in the distribution of continents and seas during Triassic times has been avoided by L. Waagen (Unsere Erde). He gives, however, no evidence for his reconstructions.

mains of which are seen to-day in Madagascar, the Mascarenes, Laccadives, and Maldives.

The alleged proofs for the existence of an equatorial, or southern, continent extending over the whole of the South Atlantic and the Indian Ocean will not stand under rigid criticism. For the maps which show a preponderance of continents over sea-basins in the southern hemisphere during Triassic times, and which misled Gregory²⁸ into his hypothesis of the turning around of the tetrahedral form to agree with the main lines of the earth's surface, we can, with greater justice, substitute another interpretation which harmonizes better with the teaching of a certain permanence of the present sea-basins.

The overwhelming majority of geologists have adopted the opinion of Suess that the Pacific Ocean is a very old one, which has remained the greatest ocean of our planet ever since Cambrian times. A contrary view has found its chief representative in Haug.²⁹ Haug on purely theoretical grounds, has arrived at the opinion that during the Mesozoic era a continent must have occupied the greater part of the present Pacific Ocean. He sees, in the geosynclines, labile zones of the earth's crust, having great piles of marine sediments, which always are laid down between great continental masses, and believes, therefore, that the great circum-Pacific geosyncline of the Mesozoic times demands a continent on its inner side.

The only support for the existence of such a Pacific continent is given by Burckhardt's proof³⁰ of a land west of the Chile-Argentinian Cordillera in Jurassic times. However, his Jurassic studies show only the necessity of postulating an island lying parallel to the Andes between 25° and 40° S. latitude, and not of an extended South Pacific continent. We may there, with Andree, (*loc. cit.*, p. 25), refuse to accept Haug's Pacific continent, and regard it as an entirely hypothetical one.

We started out with the most geocratic period of the earth's history in order to establish the maximum losses that the continents could have suffered in comparison with the sea-basins. It has been shown that the interchange between continental masses

²⁸ Plan of earth and its causes: Geogr. Journal, Vol. XIII, p. 246, 1899.

²⁹ Les géosynclinaux et les aires continentales: Bull. Soc. Géol. de France, 3d ser., T. XXVIII, pp. 6, 7, 657, 1900.

³⁰ Beiträge zur Kenntnis der Jura — und Kreideformation der Cordillere: Palaeontographica, L Bd., pp. 128, 136, 1903.

and sea-basins has at all times been kept within comparatively narrow limits. If we follow the history of the ocean still further back, it appears that as early as Cambrian time evidences are present of the existence of a Pacific as well as of an Atlantic and an Indian Ocean.

In order to reach this conclusion, we must by all means bring into use a method of study which in paleogeography as well as in geology proceeds mainly from the present relationships, and which accepts changes in the present maps only in so far as is made necessary by established biographic and geologic investigations. I believe, moreover, that only the consistent application of this method will lead us to the reconstruction of maps of the older periods of earth history which, to quote Koken, mean more than the geographic expression of the thoughts of an author. It will, I expect, make even the European geographers and geologists who up to this time have as a majority been outspoken against the permanence of continents and ocean-basins, more inclined to recognize the correctness of that theory which Bailey Willis represents, namely, that the permanence of great sea-basins stands almost outside the category of questions which are still debatable.³¹

If we concede a permanence of the major forms of the earth's crust, at least in the main — apart of course from the manifold changes in the labile mediterranean zones — then there is no longer any necessity for the acceptance of horizontal movements of salic continental blocks over the Sima of the ocean floors. The isostasy between continents and ocean-basins also exists only in the main features, not in the individual ones. No geophysical argument is opposed to the sinking of individual fragments of the continental platforms into the abyssal deeps. The evidence for the breaking up of a former continent in very recent times found in the geologic structure of the coast lands around the Aegean Sea must nullify any speculations to the contrary.³²

³¹ This thesis agrees essentially with that of Dana (Amer. Jour. Sci. (1), Vol. XXII, p. 339, 1856), from which the discussion of the permanence problem in his time started: "The continents have always been the more elevated land of the crust, and the oceanic basins always basins, or the more depressed land."

³² The observations made by Philippi on the bottom soundings obtained by the "Gauss" from the Romanche deep and from the vicinity of the Walfischrücken, with their "reversed" calcareous beds, also testify to local crustal movements on the sea bottoms in recent time.

NEPHRITE CELT FROM BAHIA, BRAZIL

BY HENRY S. WASHINGTON

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Jade artifacts are rather common in Brazil, and Simoens da Silva¹ states that they are found in several states. According to him, and to the descriptions of Fischer² and Hussak³ the jade is always nephrite, no jadeite having apparently been met with in Brazil. Nephrite objects from Brazil are rarely seen outside of that country, so that it is a pleasure to have the opportunity of describing a nephrite celt from the State of Bahia. This was found in November, 1920, by Mr. Charles E. Ivers, on the trail along the Paraguassu River, between Roncador and Andarahy, near Lencsoes, in the carbonado-diamond district. I would express my hearty thanks to Mr. Ivers for his generosity in giving me the interesting specimen, which is now deposited in the U. S. National Museum.

The small celt is of the usual form, like No. 5 of Plate I of Da Silva. Its dimensions are: length = 6.0 cm., width = 3.8 cm., and thickness = 2.1 cm. The cutting edge is not medial, but is slightly above the medial plane. The original weight was 83.3353 grams, but a piece weighing about 6.5 grams was cut out to furnish material for this study.

The celt is well finished and smooth, but not highly polished, and has something of the characteristic feel of polished jade. The general true color is a slightly yellowish gray-green, Ridgway's "pistachio green" (33"), but all of one side and part of the other is stained a dark, mottled, chocolate brown, Ridgway's "Mars brown" (13'm). The cutting shows that this brown stain is very superficial, the color extending inwards not more than about 0.1 to 0.2 mm., while the interior is of a uniform gray-

¹ Proc. XIX Cong. Americanists, pp. 229-235, 1917.

² Neues Jahrb., II Bd., p. 214, 1884.

³ Ann. Nat. Hist. Hofmuseum, XIX Bd., p. 89, 1904.

green. In this brown staining the jade resembles somewhat the so-called "tomb jades" of China, and differs from the usual surface alteration of the Amargoza (Bahia) jade, which is white, according to Da Silva and Hussak. The material is translucent, structureless megascopically, extremely fine-grained, and extremely tough, so that the preparation of the powder for analysis was a lengthy and laborious proceeding. Although the thin section shows that the microstructure is irregularly felted, yet fracture of the piece cut out indicated a tendency to split parallel to the length of the celt.

The specific gravity was determined by Dr. L. H. Adams, who obtained the value 2.946 at 21.5°, giving a corrected true density of 2.9382. The hardness is slightly less than that of quartz, or about 6.5.

The thin section shows that the microtexture is that of a typical nephrite. The rock is made up of a densely felted mass of very minute fibers of tremolite, with here and there distinct patches of slightly coarser prismoids. The fibrous and slightly patchy texture is best seen between crossed nicols. The amphibole fibers are colorless. No other mineral could be definitely made out, but there are some rare, excessively minute, black grains, probably of iron ore. No apatite was seen. The section is traversed by a rather coarse network of cracks, which have a general trend in one direction and thus give rise to the more easy fracture parallel to the length of the celt, as mentioned above.

Doctor Merwin very kindly examined the material optically. He states that the matted fibers are too fine to show singly under the microscopic; but the refractive index γ is 1.625 in those fragments which extinguish best at a maximum angle ($c \wedge \gamma$) of about 16°. The lowest observed index (α) is 1.597. These optical properties are essentially identical with those of a water-clear tremolite from Switzerland, described by Kreuz.⁴ He obtained the values: $\alpha = 1.6000$, $\beta = 1.6155$, $\gamma = 1.6272$, for the sodium line, and the specific gravity 2.980. We shall see that the chemical composition of this tremolite is almost identical with that of the Paraguassa nephrite. The values for α and γ are also close to those obtained by Ford⁵ on tremolites from Richville, New

⁴ Ref., in Zeits. Kryst., XLIX Bd., p. 213, 1911.

⁵ Amer. Jour. Sci., (4), Vol. XXXVII, p. 180, 1914.

York. ($a = 1.5992$, $\gamma = 1.6246$) and from Lee, Massachusetts ($a = 1.6022$, $\gamma = 1.6347$), which were analyzed by Stanley.⁶

A chemical analysis of the material of the Paraguassu celt, with the results is shown in No. 1 of Table I. Phosphorus and chromium were especially looked for, but not a trace of either was found. The green color must, therefore, be due to the presence of ferrous and ferric oxide. Fluorine was not determined because of the very small amount of material. The water was weighed directly by Penfield's method.

I. ANALYSES OF NEPHRITE AND TREMOLITE

	1	2	3	4	5	6
SiO ₂	59.79	54.76	62.86	58.22	57.45	57.69
TiO ₂	0.23	n. d.	n. d.	n. d.	n. d.	0.14
Al ₂ O ₃	0.88	4.08	7.24	1.37	1.30	1.80
Cr ₂ O ₃	none					
Fe ₂ O ₃	0.33	n. d.		0.04	0.18	0.00
FeO	0.96	1.80		0.61	0.22	0.55
MnO	0.10	n. d.	n. d.	0.04	0.07	trace
MgO	24.31	21.26	12.87	23.97	24.85	24.12
CaO	12.52	14.31	12.32	12.95	12.89	13.19
Na ₂ O	0.35	n. d.	4.19	0.24	0.67	0.48
K ₂ O	0.06	n. d.	trace	0.04	0.54	0.22
H ₂ O+	2.10	3.72	0.57	2.17	1.16	1.56
H ₂ O—	0.32				0.09	0.10
F ₂	n. d.	n. d.	n. d.	0.17	0.77	0.37
P ₂ O ₅	none	0.40	n. d.			
	99.95	100.33	100.07	99.75	100.19	100.22
Sp. gr.	2.938			2.980	2.997	2.980

- 1. Nephrite celt, Rio Paraguassu, Bahia. Washington analyst.
- 2. Nephrite pebble, Amargoza (Baetinga), Bahia. Hussak, op. cit., p. 91.
- 3. Nephrite celt, Philadelphia, Minas Geraes. Scheidt analyst. Fischer, op. cit., p. 215.
- 4. Tremolite, Switzerland. Kreuz analyst. Kreuz, op. cit., p. 213.
- 5. Tremolite, Richville, New York. Stanley analyst. Penfield and Stanley, op. cit., p. 31.
- 6. Tremolite, Lee, Massachusetts. Stanley analyst. Penfield and Stanley, op. cit., p. 32.

The Paraguassu nephrite is evidently a very pure tremolite, as is shown by the close similarity between its analysis and the three analyses of well crystallized, pure tremolite given in Nos. 4, 5, and 6. In all of them, it may be noted, the ratio SiO₂; (R'', R')O is very close to unity, if H₂O+ and F₂ are reckoned

⁶ Amer. Jour. Sci., (4), Vol. XXIII, pp. 31-32, 1907.

in with the monoxide bases; which is in accord with Penfield's interpretation of the role of hydroxyl and fluorine in the amphiboles. In all four the amount of CaO is slightly above the ratio $\text{CaO} : \text{MgO} = 1 : 3$. The close correspondence in refractive indices is noted above. The densities of the three crystallized tremolites are somewhat higher than is that of the nephrite, which may well be accounted for by the texture of this last.

The nephrite pebble from Amargoza shows the same general chemical characters as does our celt, although the analysis is far from good. In the Amargoza pebble the silica is lower and the lime is higher; the much higher alumina is probably to be connected with the correspondingly lower magnesia, the magnesia not having been completely separated from the alumina by reprecipitation — a very common analytical error. The high P_2O_5 in the Amargoza nephrite is in accord with Hussak's observation that apatite is an almost unfailing accessory mineral in the specimens from this locality examined by him. None of this mineral could be detected in my thin sections, and not a trace of phosphomolybdate was precipitated in my test. This would seem to be the essential chemical point of difference between the two nephrites.

Amargoza is mentioned by both Hussak and Da Silva as the only known locality in Bahia where nephrite occurs in place, as boulders and pebbles, and where large numbers of nephrite celts and other artifacts have been found. Its former name was Baetinga, used by Hussak, which means "a white object" in the native Tupi-Guarani language, referring to the white crust produced by weathering of the stones. Amargoza lies within the great belt of gneiss which traverses Bahia⁷ from north to south. It lies about 100 miles west of the city of Bahia, and about 30 miles south of Tapera, on the Paraguassu River.

The Amargoza nephrite, according to Da Silva and Hussak, shows the same green color, dense texture, and toughness as does our celt. The chief differences are that the Amargoza rock weathers to white and, according to Hussak, contains some apatite as a common accessory mineral, about one per cent in that analysed by him. In spite of these differences, however, it may be held as probable that the Paraguassu celt came from the Amargoza

⁷ Cf. J. C. Branner, *Bull. Geol. Soc. America*, Vol. XXX, p. 234, 1919.

locality, as this is the only known occurrence in the region, and is not far from the Paraguassu River.

Hussak considers that the nephrite owes its origin to the metamorphism of gabbros or pyroxenites, the amphibole being secondary. Pyroxenite and hornblendite occur near Maracas,⁸ about 30 miles west of Amargoza, intruded in the gneiss and quite fresh and unaltered; and pyroxenite is also met with near Sitio Novo on the Paraguassu. My analyses of the Maracas rocks show that they are extremely high in manganese, containing about 2 and 1.5 per cent, while the Paraguassu nephrite contains only the usual small amount. It is not probable, therefore, that the Bahia nephrite is derived from such rocks as I analysed.

The material of the Minas Geraes celt (No. 3 of the Table), for the analysis of which only about 0.2 gram was available, differs greatly in composition from the other two nephrites, and indeed from all other nephrites. The silica and soda are both remarkably high, there was a slight loss of magnesia, and Fischer regards the analysis as of only qualitative value. It is possible, however, in spite of its deficiencies, that the analysis is not far wrong, especially as regards the silica and soda. The explanation of the discrepancies may be found in what has been observed in a study of Middle American jades now being prosecuted.⁹ Most of these prove to belong to a series made up of albite and diopside-jadeite, in which the silica may be as high as 67 per cent, the percentage of soda remaining fairly constant at about 11 per cent. It is possible, therefore, that we have in this Minas Geraes "nephrite" an example of a similar series composed of albite and enstatite-diopside. Calculation shows that the analysis by Scheidt corresponds roughly to the composition: albite 35.6, diopside 47.7, enstatite 10.2, and quartz 5.9 per cent. The magnesia accidentally lost would account for some of the excess silica. The observation that nephrite (but not Jadeite) occurs along the east coast of South America, while jadeite (but probably not nephrite) occurs along the west coast of Mexico and Central America, may have some bearing on the matter.

⁸ H. S. Washington, *Amer. Jour. Sci.*, (4), Vol. XXXVIII, p. 79, 1914.

⁹ H. S. Washington, *The Jade of the Tuxtla Statuette*, *Proc. U. S. Nat. Mus.*, Vol. LX, No. 14, 1922; and *The Jades of Middle America*, *Proc. Nat. Acad. Sci.*, Vol. VIII, 1922.

TECTONIC SETTING OF LACCOLITHIC GENESIS

BY CHARLES KEYES

Incompetency of Unaided Hydrostatic Pressure. In striving adequately to account for the cause of the laccolithic phenomena presented by the Henry Mountains Gilbert¹ makes last recourse to the type of rock, the relative densities of the intruded mass and the invaded strata, and the general law of hydrostatics. Commenting upon this effort Dana² is satisfied that Gilbert's explanation is complete without reference to the differences in rock density, hence inferring that simple hydrostatic conditions are all-sufficient. Cross also, in the course of his discussion on the cause of laccoliths, is inclined towards a similar opinion, and cites in evidence the occurrence on the brink of the Grand Canyon of normal volcanoes from which lavas fell in cascades 2000 feet into the gorge. "These fissures were evidently not made by a force like that of the laccolithic eruption to the north." Although it may be conceded that with so powerful a forced movement in the lavas no other cause than that of simple and unaided hydrostatic pressure is needed for flow to any level of the invaded strata at which a fissure might terminate the fact remains that the laccolithic form of intrusion is far too infrequent in occurrence to have this agency regarded as the controlling, or sole, factor.

On the other hand are the opinions of Suess,³ Daly,⁴ Jagger,⁵ and others, that the magma during intrusion is essentially passive.

The actual association of laccolithic bodies with definite crustal rupture and with the severe localizing of some masses in the paths of marked orographic flexing, as amply shown in the Sierra del Oro, seems clearly to demonstrate beyond all peradven-

¹Geology of Henry Mountains, p. 75, 1877.

²Am. Jour. Sci., (3), Vol. XIX, p. 24, 1880.

³Sitzungsberichte der Wiener Acad., Bd. CIV, p. 52, 1895.

⁴Journal of Geology, Vol. XIII, p. 503, 1905.

⁵Twenty-first Ann. Rept. U. S. Geol. Surv., Pt. iii, p. 172, 1901.

ture that conditions other than simple hydrostatic pressure alone are often, if not always, involved.

In the original consideration of the Henry Mountains it is manifest that essential features and factors are entirely overlooked and that in the light of later observations these laccolithic masses need further critical examination.

Inadequacy of Relative Rock Densities. In the description of the Henry Mountains Gilbert lays particular stress upon the relative densities of the intruded lavas and the invaded strata. "The coincidence of the laccolithic structure with certain types of igneous rock is so persistent that we cannot doubt that the rock contained in itself a condition which determined its behavior." That this circumstance alone is not sufficient to satisfy the equations presented, is indicated by the fact that rock-type is a direct results of conditions of consolidation which take place after intrusion. As Cross⁶ so astutely points out, in both chemical and mineralogical composition the laccolithic rocks are identical with certain surface andesites. This point might be further emphasized by noting that the identity extends not only to the laccolithic mass and surface flows but to the associate dykes, sheets and other appanages.

In order that a laccolith may form it is evident that specific gravity of the magmatic mass is not competent to float the rock-prism above but that it must have had material assistance of some kind or other to produce the dome. This aid is believed to be found partly in rupture of the invaded strata thereby setting free one limb of a potential arch, and partly in local orogenic stress which initiates the flexing, the bow being maintained by competent strata in the rock-section.

Essential Presence of Crustal Lines of Weakness. The characteristic wedge-shaped outlines of many and possibly all laccoliths and the close association of these intrusive bodies with extensive fault-lines, as in the instance of the Sierra del Oro, is in itself strong presumptive evidence of a necessary genetic relationship between this class of eruptive masses and planes of profound displacement. The attendant conditions are not unlike those which obtain when one moves over thin ice, the fluid beneath not flowing out over the surface until some large crack

⁶ Fourteenth Ann. Rept. U. S. Geol. Surv., Pt. ii, p. 239, 1894.

permits the escape of the water; and if the ice be covered with snow the water insinuates itself between the layers of ice and snow.

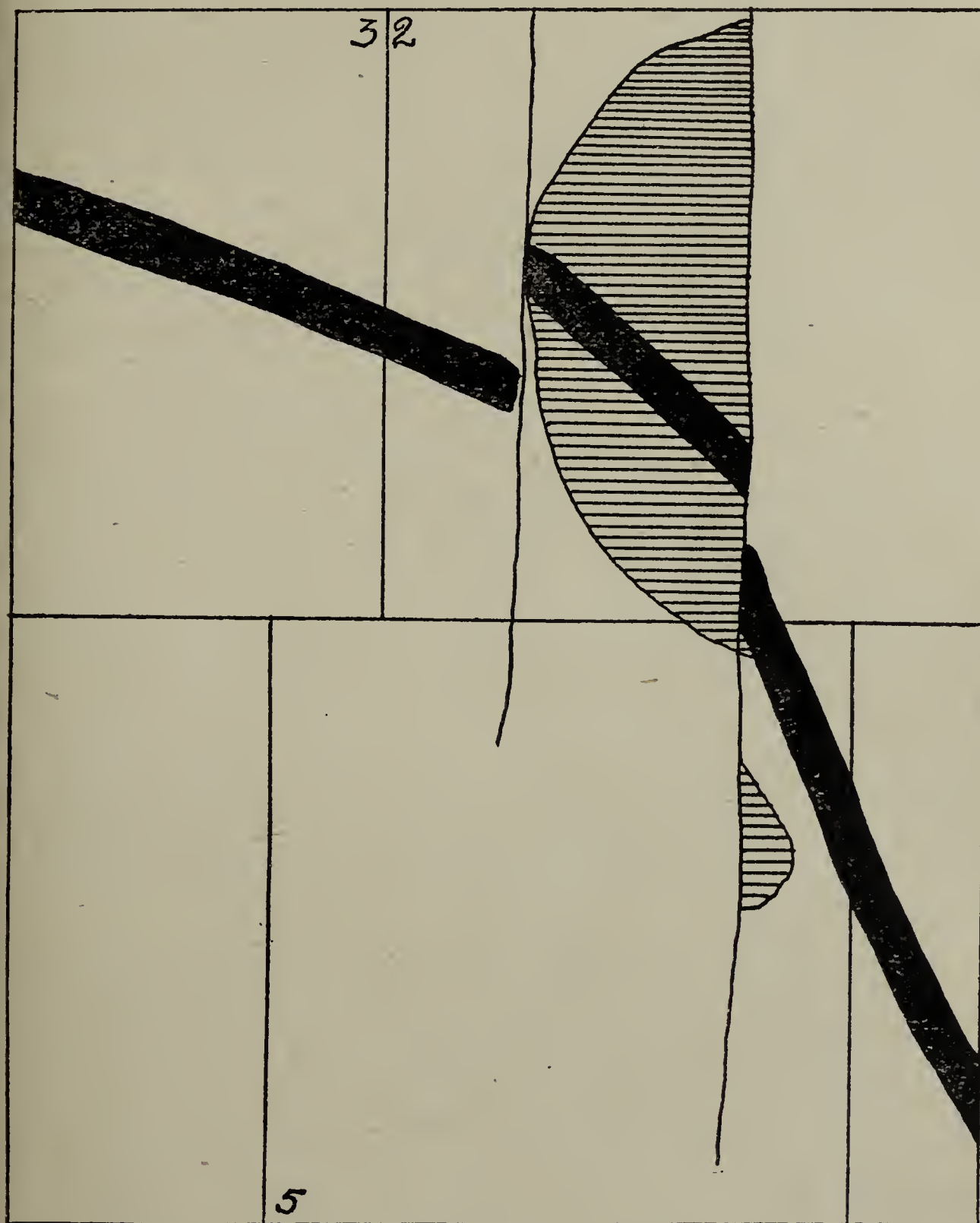


Fig. 13. Lateral Displacement of Dike.

With Cross, in the case of the West Elk Mountains of Colorado, and Iddings, in the instance of the Holmes Mountain of Montana, the possible presence of faults was a theoretical consequence. Concerning the Sierra del Oro the actual situation of laccoliths along fault-lines was a matter of direct observation

made at a time when the theoretical necessity was yet unforeseen. With the recognition of the genetic association of the two phenomena a host of other inexplicable facts is rendered readily understandable.

It may be that in crossing the old fault-line an orographic flexure creates conditions which enable the fractured supra-laccolithic strata to float upon the viscous lava, thus apparently confirming in a way Gilbert's contention of the relative densities as a controlling cause of laccolithic phenomena.

The remarkable feature of the the faulting it that the displacement may be downward beneath the level of the laccolithic mass and upward above the lava zone. This phase is particularly well displayed in the San Ysidro and the Tuertos laccoliths. In other instances the displacive movement may be cumulative as shown by the Ortiz and Los Cerrillos masses. These apparently diametrically opposite results along the same crustal rupture is probably to be ascribed to the fact that the fault-plane is a warped surface and the stress is thus torsional. The torsional character of neighboring faults that are parallel to the potential orographic axis is best indicated by the courses of great dikes which traverse the country from the laccolithic centers. Some of these dikes have a linear extent of a score of miles. One of them, near the O'Mera coal mine, seven miles south of Ortiz Station, is represented in the annexed diagram (figure 13), and plainly exhibits marked lateral movement as well as vertical displacement along its sides.

Localization of Orographic Potentialities. Beyond the topographic boundaries of a folded mountain range the flexing of strata in diminishing amplitude extends far on either side. Soon after passing the southern state-line of Colorado and in the latitude of the Sierra del Oro, the Rocky Cordillera abruptly ends. As a conspicuous anticlinorium, it pitches beneath the general plains-surface of the Mexican Tableland, never again to reappear either as a relief form or as a tectonic entity. Two great parallel folds mark this terminus of the Rockies. They are locally designated as the Las Vegas and Santa Fe ranges. Minor parallel flexures are present to the eastward and to the westward. The western corrugations extend a distance of more than 30 miles from the Santa Fe axis. These minor flexures cross the great

Sierra del Oro horst at an angle of about 45 degrees. It is at the intersection of the two that the laccoliths are situated. Four in succession are exposed and possibly others remain yet undiscovered.

The location of these laccoliths with reference to well defined lines of geologic structure is hardly fortuitous. In itself the four-time repetition of identical circumstances argues strongly for direct genetic association of the several phenomena. In the theoretical consideration of the peculiarities of the New Mexico exemplifications, in the mathematical analysis of the tectonic problems presented, and in the satisfaction of the most urgent tectonic demands an adequate *raison d'être* for laccolithic genesis seems to have been reached.

Stratigraphic Horizon of Laccolithic Intrusion. The Sierra del Oro is particularly instructive at this time because of the fact that by it are supplied some very definite and pertinent data bearing directly upon the actual depths at which typical laccolithic intrusions take place. Two wholly distinct and unrelated lines of testimony are presented. In character one trend of evidence is strictly stratigraphic. The other has to do with the physical conditions attending the genesis of normal contact deposits of the metallic ores.

From casual consideration of the great vertical section of geologic formations of the region, representing a thickness of upwards 10,000 feet above the base of the Mid Carbonic limestones, the chief horizon of intrusion, it might be expected that this figure stands for the actual volume of overburden strata. There are several qualifying features. At least two great erosion intervals intervene. One of these appears to have been at the beginning of Cretacic time, and the other at the beginning of Tertiary time. Along the belt of the Sierra del Oro horst the normal Carbonic column 3000 feet thick is reduced on account of Cretacic erosion to 300 feet. Early Tertiary erosion cuts the Cretacic section of 3500 feet to one-half.

Concerning the physical conditions attending the formation of contact-ore deposits the Ortiz and Tuertos Mountains are especially referred to.⁷ In the production of certain types of metallic ores distance from the surface of the ground is fre-

⁷ Economic Geology, Vol. VI, p. 365, 1909.

quently a prime factor. Since De Launay⁸ first called attention to the variation of metalliferous veins with depth, the theme has received wide notice. The author's main thought, and the one which is particularly suggestive, is that owing to enormous erosion which many parts of the earth's crust have undergone we are able to observe in many instances portions of veins which are actually formed at great depths. Discussing the French author's conclusions, Krusch⁹ cites a number of localities in which in relatively late geological times great denudation has taken place, the thickness of the rock-prism removed amounting to three to five miles or more. Vogt¹⁰ also supports the opinions of the writers just mentioned, but is inclined to go somewhat farther by greatly increasing the denudation units, by multiplying the ordinary figures by ten. Van Hise¹¹ although leaning towards the extreme view mentions typical examples which plainly do not support his contention, but demonstrate that the depths at which these particular contact-ore bodies appear fall very short of the zone of rock-flowage. Data recently presented on the Tuertos Mountains by Yung and McCaffery¹² furnish critical evidence upon the moot points concerning the distance from the surface at which the ore deposits were originally formed. Some of these results are personally substantiated; and others are added from the neighboring field of the Ortiz.

Taking into account the several unconformity planes in the local rock-section as representing great erosion intervals it is indicated¹³ that when the laccolithic masses were formed the thickness of the superincumbent strata could not have possibly been more than 3000 feet. In the instance of the Ortiz intrusion a thickness of 2000 feet of sediments would in all likelihood be nearer the truth for the actual volume of strata floated. These figures seem also to obtain for the neighboring Tuertos mass. The Maryville laccolith, or batholith, of Montana is believed by Barrell¹⁴ to have reached a level within 4000 feet of the surface.

⁸ *Rev. gen. des Sci. purée et appliqués*, t. XI, 1900.

⁹ *Zeitsch. f. prakt. Geologie*, p. 317, 1900.

¹⁰ *Trans. American Inst. Min. Eng.*, Vol. XXXI, p. 158, 1901.

¹¹ *Mon. U. S. Geol. Surv.*, Vol. XLVII, p. 1056, 1904.

¹² *Trans. American Inst. Min. Eng.*, Vol. XXXIII, p. 355, 1903.

¹³ *Economic Geology*, Vol. VI, p. 365, 1909.

¹⁴ *Prof. Pap. U. S. Geol. Surv.*, No. 57, p. 81, 1907.

In Utah the Iron Springs laccolith, according to Leith and Harder,¹⁵ gives a similar figure for the superincumbent load. Lindgren¹⁶ fully agrees in placing the original depth of the Tuertos intrusion at not more than 3000 feet.

Relations of Laccoliths to Sills. In the original definition it is stated as axiomatic that a laccolith is merely a thickened sheet or sill or the latter is an attenuated laccolith. The Ortiz laccolith seems clearly to display features and relationships showing that this definition is not only not necessarily correct but that the two phenomenon are formed under entirely distinct tectonic conditions. Although in the same intrusive body one part is thick and laccolithic and another part thin and sill-like the two are sharply contrasted when brought into juxtaposition, and plainly appear to have responded to rather diverse tectonics. As elsewhere indicated the main laccolithic bulging is regarded as due mainly to intrusion at a point of potential orographic stress in which the already arching strata permits the magma to localize; and also in another direction to the presence of profound faulting which tends to facilitate bowing of the rocks along a single line by allowing a rock-prism readily to swing loose as it were at one end and seemingly float upon the swelling tide.

On the other hand the intrusion of the thin sheet, or sill, appears to take place irrespective of orographic stress or faulting. In the Ortiz case the laccolithic mass abuts a fault-plane cutting a great thickness of coal-bearing shales. On the northwest flank of the mountains, at Madrid the principal coal camp, there are four thick coal-seams which together with about 350 feet of shales and sandstones lie between two great sills. Each of the latter is over 200 feet in thickness and extends a distance of several miles out from the main igneous body. The relationships of the sills to the coal-seams suggest that the intrusive sheets themselves have perhaps followed the paths of former coal-seams, possibly the most extensive of the series, as horizons along which they were able to insinuate themselves and advance most easily. Similar phenomena are not unknown elsewhere. In the Scottish coal-fields intrusives take the place of coal-seams for long distances;¹⁷

¹⁵ Bull. U. S. Geol. Surv., No. 338, p. 47, 1908.

¹⁶ Mineral Deposits, p. 647, 1913.

¹⁷ Mem. Geol. Surv., South Staffordshire, p. 118.

and in the coal-fields of Germany and Hungary Moeste¹⁸ and Vom Rath¹⁹ describe like occurrences.

The distinctive features of the sill as compared with those of the laccolith are shown in the following diagram (figure 14).



Fig. 14. Relations of Laccoliths and Sills.

MECHANISM OF LACCOLITHIC LOCATION

Concerning the cause of laccolithic intrusion Gilbert did not lose sight of certain mechanical shortcomings of his explanation. These he sought to overcome by appealing to factors which later Cross showed to be both unnecessary and not demonstrated as such. Dana got over the difficulties by brushing aside all considerations except simple hydrostatic pressure and with this feature alone regarded Gilbert's hypothesis complete. This is doubtless one of the main reasons why from a mechanical angle leading European geologists have so persistently challenged the American view of laccolithic intrusion. At the same time Old World writers on the theme offer no alternative theory to take the place of the one which they seek to discredit. In the Sierra del Oro the chief objections which were raised against the Gilbert view seem to be fully met. Controlling tectonic features which all describers of laccoliths have missed thus appear to supply the long sought desiderata.

In order that a laccolith be produced rather than any other form of volcanic manifestation it appears that the intrusive mass shall have a particular tectonic setting. Profound faulting is one of the prime factors. Another is orographic flexing by which the rigidity of certain arching strata largely maintains the load of superincumbent materials. Probably the high viscosity of acidic

¹⁸ Geologische Schiedering Meisneru. Hirschberge, 1867.

¹⁹ Neues Jahrbuch, f. 1880, p. 276.

magmas has an important but yet uncalculated influence on events. The horst structure of the Sierra del Oro carries the matter a step more remote and explains the deep-seated cause of the major faulting whereby an orographic prism is sustained by a sharp pre-Cambrian arch the rigidity of which is not yet lost, while the adjoining blocks on either side are allowed to slide down, as it were, the steep sides of the old flexure.

In the course of the uncovering of laccolithic masses through erosion different groups display different stages of advancement. The Ortiz Mountains represent a state of depletion in which the covering is completely removed and the unearthed body is so profoundly dissected that the mass is almost nearly cut through to its foundations. San Ysidro still retains part of its cover. The laccolithic masses of Los Cerrillos, in New Mexico, and Mount Marceline, in Colorado, are even less exposed. Henry Mountains seem to be only in the initial stages of revelation. Some of the outlying mounds of the last mentioned group have merely hypothetical existence. Considering this stage alone it is small wonder that Gilbert hit upon the simplest explanation, and could not at the time at which he made his observations fancy any structural dependence of the all but hidden masses. His blister explanation was as simple and as natural as could be, and when he came to make the model of the Henry Group and a restoration of its features the quaquaversal mounds were a necessary consequence of his hypothesis.

Later realization of such possible complications of the problem as the Sierra del Oro structures present perhaps accounts for Gilbert's strong desire, just before his demise, to review the structures which he had made so famous, before passing final judgement on the theme of his early efforts.

It may be noted in this connection that close inspection of the original ground-plan of the Henry Mountains, in the light of the Sierra del Oro structures, discloses a possible line of dislocation, like in the case of the New Mexico example, parallel to the axis of the great Water Pocket flexure, a short distance to the west of the Henry Group. Probably the Henry Mountains should be revisited, and their ground-plan redrawn along tectonic lines, if possible, for surely these mountains do not have mere fortuitous setting. Perusal of the published description of Henry

eminences gives rise to the suspicion that all of their story is not yet told.

RECAPITULATION

From the foregoing account it appears that:

- (1.) The simple Blister Hypothesis of laccolithic formation is entirely untenable;
- (2.) No laccoliths are probably regular lenticular masses haphazardly disposed;
- (3.) Unaided hydrostatic pressure alone is rarely, if ever, competent to produce notable interstratal swelling and filling by normal magmas;
- (4.) The type form of laccolithic mass is an asymmetric or wedge-shaped body;
- (5.) The super-intrusive load is severely limited to a very moderate columnar section, perhaps never exceeding 3000 feet of strata;
- (6.) Laccolithic intrusion is genetically associated with profound crustal rupture and displacement;
- (7.) The initial impetus to laccolithic formation is a release from orographic strain involved in local folding whereby the load of superincumbent rocks is, potentially at least, maintained by the rigidity of beds comprising an arch;
- (8.) The tenor of magmatic viscosity is doubtless a very appreciable and perhaps measurable factor in the production of laccolithic bodies;
- (9.) Laccoliths are not in any sense merely thickened sills or transformed dykes, but owe their especial expression to entirely distinct and widely different tectonic conditions;
- (10.) The formation of laccoliths is a necessary consequence of a combination of several unrelated conditions and circumstances operating successively or in conjunction.

NATURAL BRIDGING IN THE HIGH PLATEAUS

BY PROF. FREDERICK J. PACK

University of Utah

The Natural Bridge of Virginia has been so long regarded as one of the famous relief features of this continent and of the earth that it has come to be considered as the seventh wonder of the world. Utah has many such natural bridges. By side of Virginia's pride Utah's bridges are simply colossal. Some of them are the longest natural spans in existence. Despite the fact that so many of these bridges are already under observation in this most inaccessible Plateau country others doubtless remain yet to be revealed. Further exploration may not possibly result in the discovery of structures equal in stately magnitude and imposing grandeur to those now known but it is sure to disclose many more of close secondary importance.

The country in which the great bridges are situated is so inaccessible and irregular that the explorer easily passes within a few rods of them without suspecting their existence. Cattlemen ranged their stock in the vicinity of the bridges of San Juan County for nearly twenty-five years before they discovered them. None of their spans were probably seen by white man until 1883; and even then Indian guides led the way. The incomparable Nonnezoshie arch is said to have been first visited by an Indian so late as 1909. Then again, both of the bridges situated within less than fifteen miles of Cedar City, were first seen only about five years ago.

All of the natural bridges here considered are situated in southern and southeastern Utah. Two of them are in Iron County, and the others are in San Juan County. The accompanying outline map (Fig. 15.) indicates the locations and the routes of travel. The mammoth Nonnezoshie arch, because of its peculiar structure and size, is in a class by itself.

Both of the bridges in Iron County are situated within fifty miles of the railroad; and during the greater part of the year can now be reached by automobile. Those in San Juan County

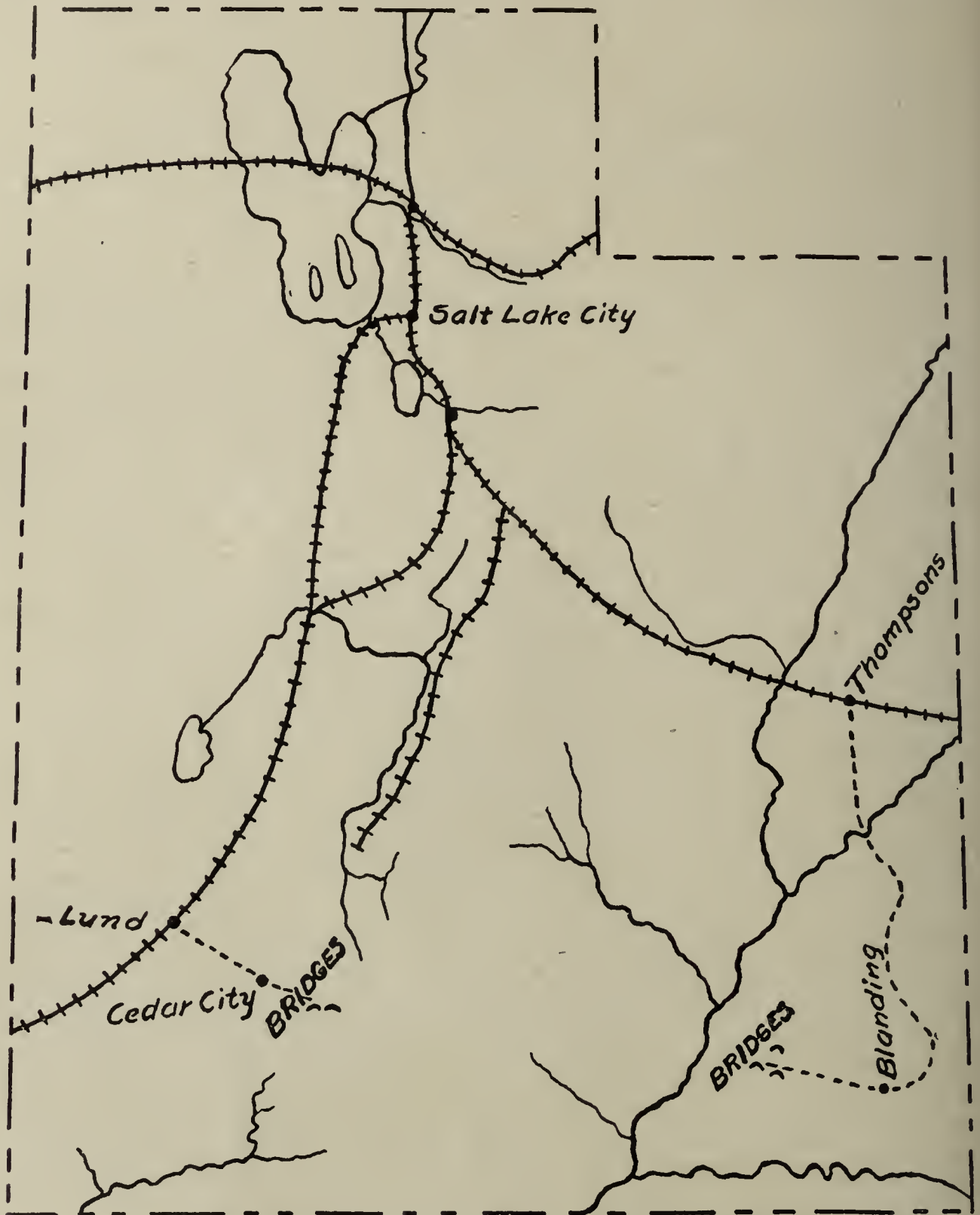


Fig. 15. Locations of Natural Bridges in Utah.

are somewhat less accessible, being 160 miles from Thompsons, the nearest railroad point in the State. One hundred and twenty miles of this distance is covered by automobile road, and the remaining 40 miles must be traveled on horseback. Efforts are now under way, however, to make this section of the road passable to

automobiles. The Iron County bridges are situated far up on the western flanks of Markagunt Plateau; while those of San Juan County occupy entrenched positions in the top of a vast platform.

The general geological features of southern Utah have to be viewed in their relations to those of the whole state. The Wasatch and the Uinta mountains of northern Utah were brought into existence by the Laramide revolution of Late Cretacic times; and that the High Plateau country to the southward was elevated toward the close of Tertic times. Moreover, the two areas not only differ in point of age, but more particularly in point of structure. The formations of the mountainous section are almost everywhere highly folded and contorted; while those of the Plateau country occupy nearly horizontal positions. Topographically the northern area is sharp and serrate, characterized by V-shaped canyons, here and there modified by the effects of Pleistocene glaciation. At the south the typical uplands are flat-topped and the incisions are of the steep-sided box-canyon type.

The late Captain Dutton calls attention to the stupendous amount of erosion which has taken place over the Colorado Dome country since the time of its uplift. According to this explanation the thousands of square miles comprising the Colorado platform proper have been stripped of all the formations from the Tertic to the Carbonic Periods, comprising an aggregate average thickness of close to ten thousand feet, and that the higher plateaus at the north and north-east are simply peripheral remnants of the one-time almost infinitely larger mass.

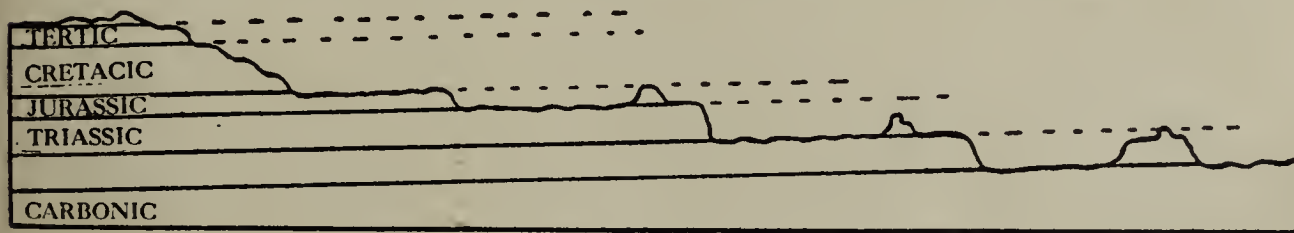


Fig. 16. Grand Stairway of Utah.

Whether Dutton was fully justified in his far-reaching conclusions may forever lack final evidence, yet it cannot be doubted that at least large vertical sections along the northern margin have been denuded much as he suggested. Proof of this is seen in the almost numberless outliners, the great stairway of withdrawing cliffs, and the beheaded streams on top of the residual

plateaus. The accompanying sketch (Fig. 16) gives a generalized north-south cross-section showing the principal geological conditions; also the manner in which the topographic divide migrates as the cliffs recede.

Southward-trending streams flow against the dip of the beds and as they approach each cliff they necessarily incise themselves deeper and deeper into the formations. Then, as they pass beyond the cliff, they travel in the open for a short distance and again bury themselves in canyons of gradually increasing depth. Thus it will be seen that contemporaneous with the recession of the cliffs, the streams embed themselves into successively lower formations, and by so doing retain many of the characteristics which they formerly possessed. Probably some of the most spectacularly entrenched meanders in the world are present in this southern Utah region.

The Mormon town of Blanding is the outfitting point for the last forty miles of the trip to the San Juan bridges. It is situated at an elevation 7000 feet above tide, and immediately at the base of Blue Mountains. From this point the trip is made on horseback. In traveling westward from Blanding to the bridges, one passes over a great platform having a geological structure quite similar to that shown in figure 16, similar in the sense that one travels alternately against the dip of the formations and down steep escarpments, but dissimilar in that the two distal places — the outfitting point and the bridges — are practically at the same elevation. At Blanding one starts on rocks of Cretacic age, and at the bridges one has stratigraphically descended to the Triassic level, and yet has maintained an equal topographic position. This, of course, means that the beds dip toward the east at rather low angles.

The highest point between Blanding and the bridges is reached about midway between the two places, specifically where the trail passes over a broad uniclinal structure locally known as Elk Mountain. The eastern face of this upland constitutes a long gradual slope, while the one on the west is sharp and abrupt. Kigalia, a ranger station, situated near the crest of this mountain and right in the midst of a primitive forest, affords a wonderful camping place for all who go that way. The greater part of the country between Blanding and the bridges is arid, maintaining

at most a fair growth of cedars, but in the vicinity of Kigalia the mountain is covered with a dense growth of timber, many of the pines being from four to six feet in diameter and in excess of a hundred feet in height.

From the top of Elk Mountain looking toward the west, the traveller obtains his first view of the platform in which the bridges are situated. It lies more than a thousand feet below him and stretches out toward the south and south-west almost as far as the eye can see. When thus viewed from the distance, this vast, level plain appears to be practically unbroken, except by outliers, large and small, which rise abruptly from its surface, like so many flat-topped icebergs from an undisturbed sea.

Those who have been fortunate enough to have the privilege of looking down upon a broad valley filled with heavy clouds, can readily imagine the view from a point of vantage on Elk Mountain. When seen from afar, the great white sandstones — likened to the clouds in the valley — at first appear to form an unbroken floor over which the traveller might pass, but when examined more carefully, the presence of steep-sided, illuring canyons is plainly discernible.

This vast platform is so completely dissected by an intricate labyrinth of box-canyons that passage directly across it is wholly impossible. Not infrequently the explorer is compelled to travel many miles around circuitous cliffs in order to reach a desired point only a few rods distant from the place of beginning. Even though the canyons in this particular locality seldom exceed an average depth of 500 feet, yet their walls are so nearly vertical as to make them effective barriers even to a man afoot.

Thus we have before us the home of the greatest natural bridges in the world. Entirely obscured from distant view, and far below the general surface of the surrounding country, these masterpieces of Nature are hidden away in one of the most inaccessible countries known. Their security and isolation might almost be interpreted to mean that Deity had deliberately decided to place them in such a position that only those who were willing to pay the price of distant travel shall be permitted to look upon them.

After leaving the top of Elk Mountain the traveller descends its steep, western face by a zigzag trail, and in course of time

reaches the headwaters of what farther down is a mighty chasm. Almost immediately after the canyon is entered it becomes so deep that the level country, into which it is carved, is lost to view, and the traveller's vision is limited by the steep walls of the meandering defile. Occasionally the trail leads directly along the channel of the intermittent stream, and at other times it climbs to the flanking cliffs in order to avoid waterfalls or other obstructions.

Six or eight miles below the point where the trail first enters the canyon, the great Edwin bridge is situated. This is in Armstrong canyon. The Carolyn bridge is four miles below the Edwin bridge and immediately at the junction of Armstrong and White canyons. From here the Augusta bridge is situated two miles up White Canyon. It will thus be seen that the three bridges occupy the apices of a triangle of two to four miles on the side.

As incidentally pointed out in a preceding paragraph, the rock formations of this particular area consist essentially of light-colored Triassic sandstones. It is more clearly accurate, however, to describe them as buff and light-red in color rather than as white or whitish, except perhaps as seen from a distance. The upper one to two hundred feet are characterized by pronounced cross-bedding, much like that of the Jurassic sandstones farther to the north and west. Immediately beneath this member is an unusually massive sandstone free from interbedding and fractures. Next below is a thin stratum, 4 to 8 feet thick, of easily weathered argillaceous sandstone. This in turn is followed by another massive member which continues to the bottom of the canyon.

The origin of the bridges is, of course, intimately related to the geological formations in which they occur, and, in consequence, the essential structural features should be kept clearly in mind. In this immediate connection by far the most important members are the two massive sandstones and the intercalated weak clayey layer. All of these formations occur in the lower depths of the canyons and are virtually identical at all of the three bridges mentioned.

All of the streams in this region are deeply entrenched and most of them are very crooked. Although all of the bridges owe their origin to stream-mandering, yet none of them has arisen

from the normal operation of this principle, except perhaps the Augusta bridge, and even in this instance there are some modifications.

In the main description of any one of the bridges would suit equally as well for any of the others. Each of the three bridges mentioned is situated near the confluence of the main stream and one of its tributaries. Furthermore, in two cases the tribu-

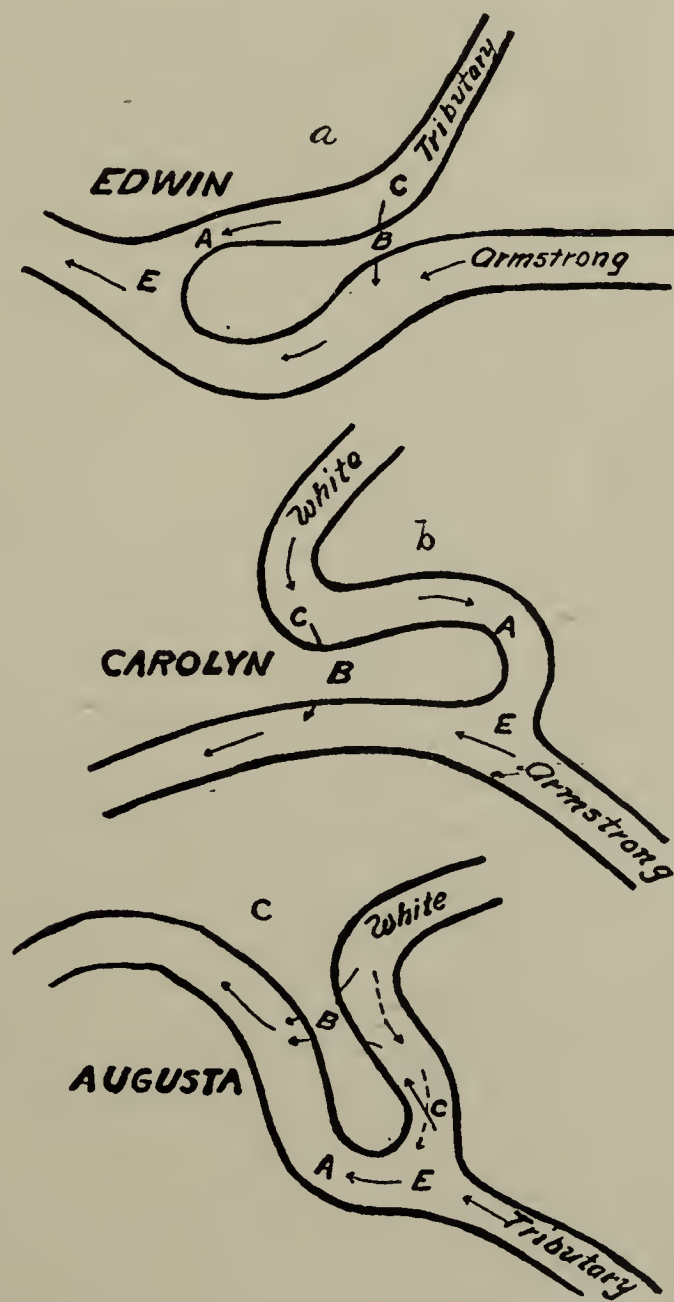


Fig. 17. Genesis of Natural Bridges.

tary joins the principal stream at a small angle, thus leaving a long and comparatively narrow neck of higher land between them.

Throughout the earlier stages of stream-entrenchment, and while the canyons were still being cut into the upper massive sandstone, meandering was not easily accomplished; but when the soft, argillaceous member was encountered, the channel quickly became

more and more tortuous, often forming pronounced recesses beneath overhanging cliffs, that resembled huge sounding-boards built for out-of-door entertainments. Ideal conditions were furnished for the formation of a bridge when two of these recesses were produced, one on each side of a narrow neck (Pl. xii.).

By reference to figure 17 it will be noted that White Canyon formerly made a sharp meander just before reaching Armstrong Canyon and approached the latter, somewhat abnormally, at an up-stream angle. By the time the channels had entrenched themselves to the depth of the argillaceous sandstone, the combined action of the two streams undercut the formation at the neck B, and permitted the water of White Canyon to flow under the bridge as indicated. The abandoned stream channel at the bend A is slightly less than 100 feet above the stream-bed at C. Naturally, its level is also approximately the same as that of the argillaceous sandstone.

The conditions at the Augusta bridge are slightly different. By referring to figure 17c it will be seen that a tributary formerly entered White Canyon just at the outward swing of a sharp meander. After the White Canyon stream had imbedded itself to the proper horizon, it worked its way beneath the narrow neck B in much the same manner as had been done at the other bridges. Just here, however, an unusual thing occurred. Prior to the time of the formation of the bridge, the White Canyon stream passed the point C in the direction indicated by the dotted arrow, but after the principal stream had deserted this section of its channel, the one coming in from the tributary reversed the former direction of flowage at C and now travels as shown by the full arrow.

The conditions which produced the Edwin Bridge differ only in detail, rather than in principle, from those which produced the Carolyn Bridge. Again referring to figure 17a, it is noted that in the immediate vicinity of the Edwin Bridge the tributary formerly entered Armstrong Canyon at the point marked E, but later, because of meandering of both streams, it broke under the narrow neck — at the horizon of the argillaceous sandstone — and immediately deserted its channel at A. Thereafter it flowed under the bridge as indicated by the arrows at B. The abandoned part of the channel at A is now 50 feet higher than the stream-

bed at C. These conditions afford evidence of two interesting points: First, the level at which the stream broke through beneath the narrow neck, and second, the amount of erosion that has subsequently taken place.

At all of the three bridges the old deserted channels are almost as easily traceable as if the water still flowed in them (Plate xiii, A). Since the time of the formation of the bridges, however, the used channels have been cut from 50 to 100 feet deeper, and, in consequence, the deserted sections are left stranded at a correspondingly higher position.

Plainly, the elevation of the abandoned channel marks the horizon at which the stream broke through and initiated the archway. With this fact in mind it is not difficult to determine the relative rate at which the opening has been enlarged, downward and upward. In the case of the Edwin Bridge and also of the Augusta Bridge, the development in both lateral directions has greatly exceeded the upward.

The enlargement of the archway downward is, of course, accomplished very largely by stream-action alone. It is altogether possible, however, that when the opening was small, wind played an important part in its upward development; but, on the other hand, as it increased in height this factor necessarily diminished, until at present it is almost entirely replaced by spalling brought about by both freezing and daily changes of temperature.

Aside from the value of this fact in the present connection, it is interesting to note that in a country such as this, the down-cutting action of an intermittent stream is just about equal to the up-cutting accomplished by spalling on the lower surface of the overhanging rock-mass.

Notwithstanding the fact that wind action on the under side of the bridges is at present so slight as to be almost negligible, yet, on the upper surface, it is of far more than passing importance. Circular wind-pits, ranging from one to four feet in diameter, are very abundant, particularly on the top of the Edwin Bridge, where they comprise an intimate mass of shallow borings covering half the entire surface. In addition to the eroding action in connection with the wind, these depressions act as temporary receptacles for water derived from precipitation, and as places of marked disintegration during freezing weather.

Measured from the angle of development, the Carolyn Bridge is by far the youngest of the three (plate xiii, B). The archway is comparatively small and the bridge itself is heavy and massive. The span of the arch, measured from abutment to abutment, is 186 feet. The bottom of the arch is ninety-eight feet above the stream-bed; and the height of the bridge, measured over all, is 205 feet. The width of the roadway is 49 feet; and the depth of the arch is 107 feet.

Next in order of development is the incomparable Augusta Bridge, whose span is 261 feet, and whose height to the bottom of the arch is 157 feet. The total height from the creek bed to the top of the bridge is 222 feet. The width of the arch is 28 feet, and its thickness is 65 feet. Although not nearly so massive as the Carolyn Bridge, the Augusta Bridge is superior not only in point of size but particularly in the qualities of form and symmetry.

So far as is known this is the largest natural bridge in the world. (The Nonnezoshie is an arch.) Its height would permit an ordinary fifteen-story business building to stand beneath it. The mighty structure, the gorgeously colored rocks and the intense greenness of the vegetation all combine to make the *ensemble* a most impressive sight, and one never to be forgotten.

The Edwin bridge is the oldest of the three structures (plate xii). The bridge itself is chiseled down to a mere skeleton of its former massiveness. The thickness of the spanning column is slightly less than ten feet at its middle point, and not more than 25 feet in width. The arch has a span of 194 feet, and an elevation of 108 feet. The whole structure is so slender as to give one the impression of great insecurity, but, of course, barring some unlooked for cataclysm, it will probably not collapse for many centuries (plate xv).

The three bridges constitute a nearly perfect series showing the normal sequence of development. The Carolyn Bridge is still in its early formative period. The sublime Augusta Bridge is at its prime (plate xiv, B). The Edwin Bridge already reaches its old age, and while observers feel that this bridge excels the others in point of grace and beauty, yet it cannot be denied that the centuries of its existence are rapidly drawing to a close.

Actuated by the hope that somewhere in the vicinity of the

present bridges a structure still older than the Edwin Bridge might be found, the writer recently (1921) made an investigation of the area and was rewarded by finding the remnants of a collapsed bridge, situated in White Canyon about two miles above the Augusta Bridge. The abutments of this bridge are still standing, one of them in an excellent state of preservation, and the other badly disintegrated and largely removed. The greater part of the collapsed arch has been carried away by the stream across the channel of which it had fallen. In point of size this bridge had a span as great as that of the other bridges, but its height was somewhat less. The same evidences of origin, in the form of undercutting stream-action and the abandonment of certain parts of the old channel, are present here as elsewhere. As a name for the remnants of this mighty structure is suggested the "Fallen Monarch."

The "Natural Bridges National Monument" was created by official proclamation in 1908, under the administration of President Roosevelt. In 1909 the boundaries were somewhat enlarged and Hopi Indian names were given to the bridges as follows, Kachina (Carolyn), Sipapu (Augusta) and Owachomo (Edwin). The older names, however, had become so thoroughly entrenched in the minds of the public that these official designations were never widely employed, and, in consequence, were soon lost sight of. It is believed that any effort to bring them into general usage would be of questionable value.

The Iron County bridges are not to be thought of as possessing the same majestic grandeur such as characterizes those in San Juan County. They are not nearly so large, and perhaps in some respects not so graceful, but on the other hand, they possess certain points of distinction that easily justify their being mentioned in connection with bridges of the other type.

Cedar City is situated immediately at the western base of the Markagunt plateau, and 35 miles from Lund, the nearest station on the Los Angeles and Salt Lake railway. In this vicinity the regularity of the western face of the plateau is interrupted by the presence of a deeply eroded canyon and its tributaries. This is Cedar Canyon. Eight miles above its mouth the canyon bifurcates. The south fork contains a good automobile road on through to the top of the Plateau. The north fork, however,

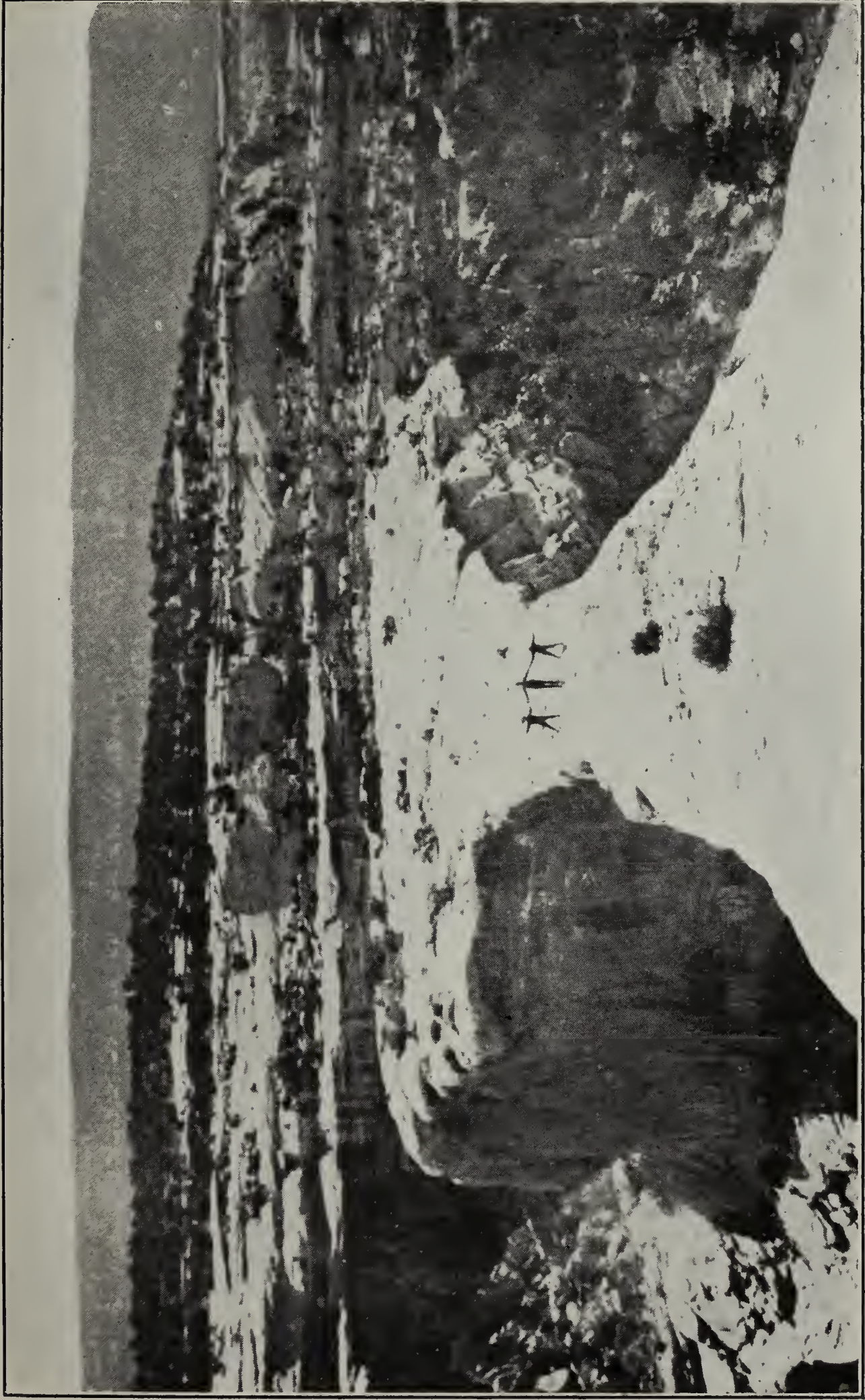
called Ashdown gorge, is impassible except on horseback. The waters of this gorge collect far up near the rim of the Plateau in a broad amphitheatre called Cedar Breaks. The automobile road in the other tributary leads to the plateau and then along the rim to a point where Cedar Breaks can be seen from above.

The Plateau in this vicinity attains an average height of nearly 10,000 feet, while its base, at Cedar City, less than 15 miles distant, is 4,000 feet lower. The strata are characteristically horizontal, except at the western base where they are highly tilted toward the east, due to proximity of the great Hurricane Fault. The sedimentary rocks exposed on the western face range from Carbonic to Tertiary age, with a lava flow on the top. The varicolored formations exposed in the lower part of the canyon alone are easily sufficient to attract widespread attention.

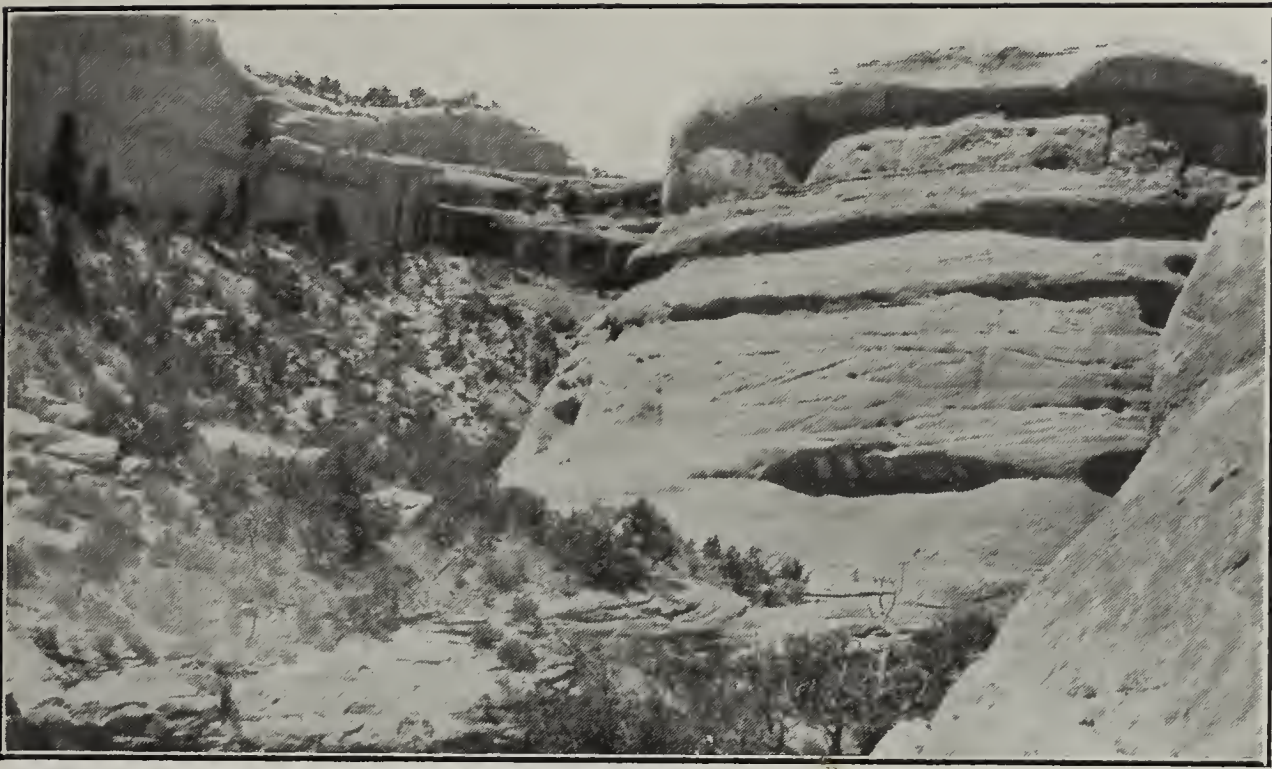
Ashdown Gorge constitutes a narrow defile, scarcely more than a hundred feet wide and nearly a thousand feet deep. Throughout its length of three or four miles the gorge is only sufficiently wide to accommodate the stream. No tributaries enter it, either large or small, except at heights far above the level of the canyon floor, much in the manner of hanging valleys in a glaciated region. This condition, however, is wholly due to water erosion. The tributaries have not been able to keep pace with the downward cutting action of the main stream. Both above and below this place the canyon loses its typical box-type and widens out into one of receding sides.

At a point about half way through the gorge and nearly a thousand feet above the stream channel, one of the tributaries from what we may call a hanging valley, has produced a beautifully proportioned bridge (plate xvi, A). The span is 100 feet long, and the arch is slightly less. In order to see this structure from the stream-bed below, one is limited in his selection of position to a very few square rods, and even then he must look upward at an angle closely approaching ninety degrees. The bridge is hidden away in the midst of a multitude of abrupt crags, themselves covered with a dense growth of pines. In such an environment it is small wonder that such a bridge was not seen until five years ago, even though it is situated within ten miles of a flourishing settlement.

In general form, Cedar Breaks might be likened to the cirque



FLOOR OF THE EDWIN BRIDGE IN SAN JUAN COUNTY, UTAH



A. Deserted Stream Canyon near Natural Bridge.



B. Carolyn Natural Bridge
UTAH NATURAL BRIDGES

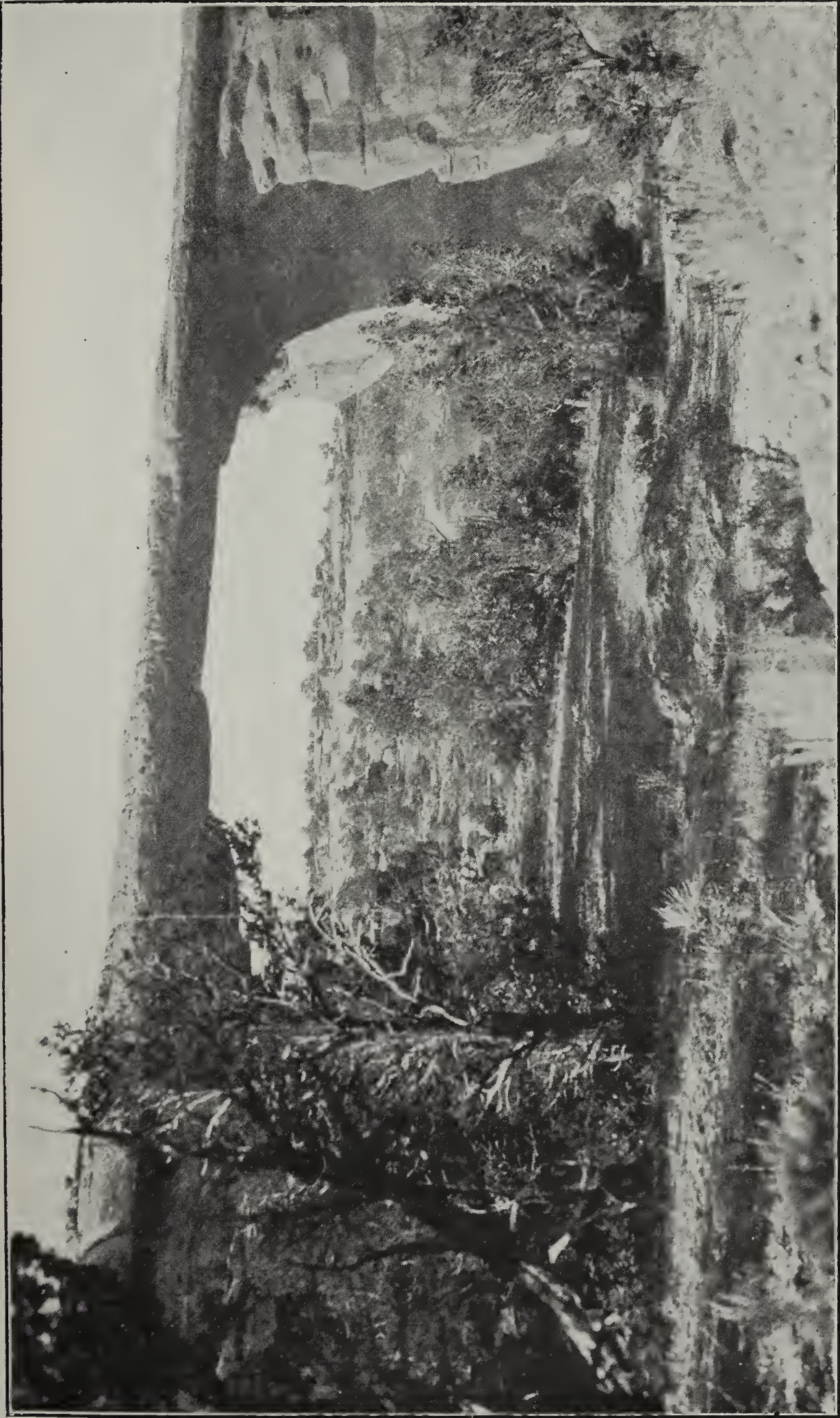


A. Profile of an Ashdown Gorge Bridge.



B. Augusta Bridge

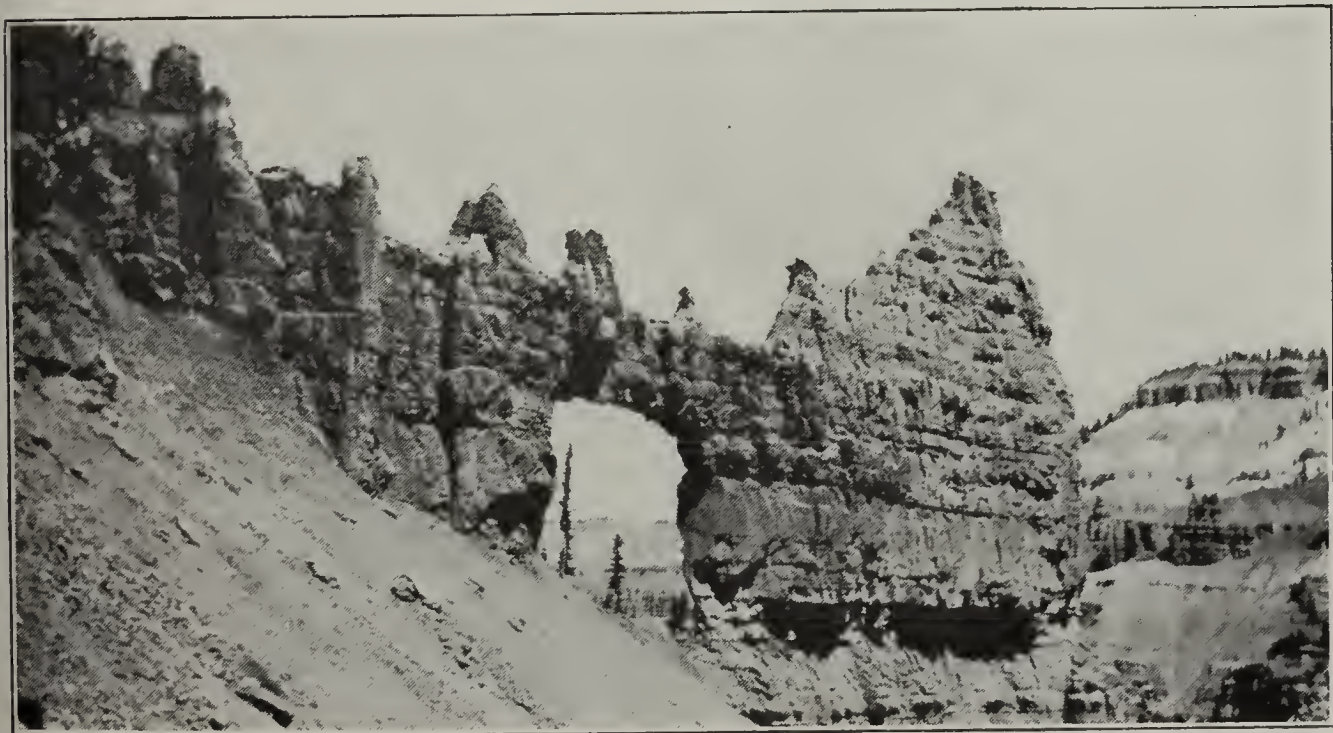
NATURAL BRIDGES OF UTAH



EDWIN ARCH IN SAN JUAN COUNTY, UTAH



A. Natural Bridge in Ashdown Gorge.



B. Walls of Jericho, in the Cedar Breaks.

NATURAL ARCHES OF IRON COUNTY, UTAH

of a broad, hanging glacier. Its existence, however, it due to water action, aided by frost and wind. When seen from a distance, its freshly eroded surfaces give the impression of a landslide, hence the name "Breaks." The amphitheatre is nearly ten miles across and all its streams converge to a point, about a thousand feet below the rim, like the spokes of a badly "dished" half-wheel. Both from above and below this vast area is wholly inaccessible except to skilled climbers.

Cedar Breaks is destined to become known throughout the world, chiefly because of two outstanding features—its wide variety of grotesque erosional forms and its incomparable coloring. The formations composing it are soft argillaceous limestones of Tertiary age. Rapid differential weathering has carved out literally a myriad of shapely and shapeless figures, among which the observer can imagine any form toward which his fancy may turn.

Any attempt to describe the coloring of Cedar Breaks leads one into the use of so many superlatives that the average reader feels justified in concluding that the matter has been greatly overdrawn. In the present connection it is sufficient to state that one day the writer and half a dozen friends sat on the rim of Cedar Breaks and successfully challenged each other to name any shade or tint of the spectrum that could not be seen in the banded formations before them.

Right in the midst of this glorious spectacle of painted fantasies is the "Wall of Jericho" (plate xvi, B). This unusual structure is scarcely a typical bridge, and yet it possesses most of the essential features. On the other hand, it more closely resembles an opening or gateway in an artificial wall. The residual mass of which it forms a part, is the result of differential weathering, chiefly wind, aided by frost and running water. Neither its height nor its span greatly exceeds 50 feet, yet its coloring, its irregularity of form and particularly its environment give it a peculiar distinction not present in the other bridges.

ORE-DEPOSITION IN TRUNK-CHANNELS OF CIRCULATORY GROUNDWATERS

BY CHARLES KEYES

Although ore deposits so often occur along the restricted lines of free-flowing groundwaters it does not follow, as we have been so commonly led to believe, that the presence of ore-bodies in such situations is solely due to the long continuance of unusually great volumes of waters passing by. Ore-localization is almost invariably determined by other and entirely unrelated factors. As recently and conclusively shown it is only under especial geologic conditions that any parts of underground trunk-channels become commercial ore-repositories. As *loci* of ore-deposition altogether too much importance appears to have been attached to these subterranean streams. The conception is very largely a purely theoretical one; and has its foundation in certain analogies drawn from the field of general rock-metamorphism.

In contradistinction to slow and general groundwater circulation trunk-channel movement of course refers to relative rapid flowage through more or less open passages such as are occasioned by faulting, shearing or solution enlargement along joint-planes. The broader signification sometimes attached to the term that regards trunk-channels as equivalent to any section of a terrane through which groundwater moves seems directly to contradict the more generally accepted definition.

Practical consideration of the relations existing between ore-deposition and groundwater circulation indicates that ascending and descending movements are not the features to be particularly contrasted. Only in a very limited sense are these two distinctions of practical service. The fundamental comparison to be made is between the trunk-channels of the vadose zone and those of the profound region. Soon after passing below ground-

water-level descending meteoric waters do not appear to be notably ore-depositing. Only in very exceptional cases should ore-materials be precipitated from such waters after they have attained considerable depths or have begun to travel upwards. Artesian waters derived from the general groundwater circulation seem to be remarkably free from metallic constituents. On the other hand metalliferous ascending waters appear to be invariably associated with vulcanism and orogenic movement. De Launay,¹ especially, has lately emphasized the idea of a close genetic relationship between orogeny, petrology and metallogenesis.

Clearly distinguishing the well-defined channels of free-flowing subterranean waters which occur in the vadose zone and the trunk-channels of the profound region the features to be especially contrasted are: (1) the sources and nature of the ore-materials carried; (2) the manner of precipitation; (3) the attitude of the rock-spaces, since in the vadose zone the trunk-channels are mainly disposed horizontally or nearly so, while those of the profound zone are usually almost vertical; (4) the mineralogic changes from point to point which the metallic content of circulatory waters undergo, since vadose waters as they sink beneath groundwater-level at once loose a considerable part of their load, but waters rising from the depths part with most of their metalliferous components long before reaching groundwater-level; and, (5) the wide differences of attendant physical conditions — vadose channels being mainly paths of rock-solution, those of the profound zone being principally lithoclastic in origin and paths of cementation; and all being quite ephemeral.

The common notion appears to be that the main channels of groundwater circulation are vertically disposed, and that in some places the water-currents are ascending but in other descending. So far as ore-deposition is concerned this is only true under certain restricted conditions. On the whole the trunk-channels of the profound zone that are ore-depositing are probably vertical or nearly so, yet relatively few of them are productive of workable ore-bodies. It is another and entirely distinct problem whether such ascending waters carrying depositable ore-materials are expelled from deep-seated magmas which are cooling, or whether they are really meteoric waters which have been heated in their subterranean travels.

¹ Cong. géol. International, Xème Sess., Mexico, p. 555, 1907.

The recent exhaustive arguments of Brun² and of Stutzer³ for the anhydrous character of magmatic emanations appear to afford no adequate explanation of the presence of water in many silicate minerals of plutonic rocks, nor of the peculiar relations existing between such rocks as massive granites,⁴ pegmatitic dikes,⁵ and quartz-veins.⁶ My own opinion has been that ore-bodies deposited by ascending waters are derived very largely, if not entirely, from juvenile waters, rarely if ever from heated meteoric waters. Nowhere so far as the profound zone is concerned have I been able to find satisfactory evidences of the lateral secretion of ore deposits even in the broadest sense of that term.

The chief diversion of vadose waters in a horizontal direction is productive of two notable results. A large gathering-ground for diffused ore-materials in decomposing rocks is drawn upon. These ore-materials are soon directed along restricted paths and, without great losses, are often carried long distances.

Assuming as fact, as Posepny⁷ has so well urged, that groundwater-level is generally an inclined plane, vadose waters are in consequence continually moving down this slope, sometimes fast through open crevices in the rocks, sometimes slow through almost impermeable masses. In its larger aspects vadose ore-formation is comparable in a way to the concentrations of powdered ore on the Wilfley-table—the uprising of mountains tilting both the former slight incline of the groundwater-table and the strata so that all meteoric waters falling upon the area are directed along certain definite lines or restricted troughs. Whenever geologic structures assume the character of cross-folds, faults, or other obstructions to the free movement of subterranean circulation, impounding conditions occur and the metallic loads in solution are at once precipitated to form ore-bodies. The tectonic cross-bars are thus the analogues of the riffles of the Wilfley. In comparison with the precipitation of metallic minerals through impoundment of groundwaters all other methods of vadose ore-deposition are perhaps quite insignificant.

In some regions, as the Ozarks for example, it has been shown⁸

² *Recherches sur l'exhalaison volcanique*, Paris, 1911.

³ *International Kong. Düsseldorf*, Anteil. iv, Vortrag 21, pp. 1-8, 1910.

⁴ *Fifteenth Ann. Rept., U. S. G. S.*, p. 721, 1895.

⁵ *Ibid.*, p. 679.

⁶ *Les Eaux Souterraines aux Epoque Anciennes*, par A. Daubrée, p. 123, 1887.

⁷ *Trans. American Inst. Mining Eng.*, Vol. XXIII, p. 213, 1894.

⁸ *Trans. American Inst. Min. Eng.*, Vol. XL, p. 205, 1910.

that trunk-channels are lines of rapid ore-solution and removal rather than of ore-deposition. In reality the Ozark'Country is a region which is now being very rapidly depleted of its once rich ore-materials. Only when the main channels of groundwater flow become clogged in some way or other do ore-bodies now form in them. To this fact more than anything else is it due that ores are not everywhere evenly deposited in the vadose zone. It is likely that it is a fundamental law of groundwater movement that only where the movement is interrupted, or stagnation prevails, are ore-materials precipitated and localized.

Still another feature has particular bearing upon this topic. Notwithstanding the fact that vadose waters generally tend not to drop their metallic loads in the rock-cavities through which they rapidly pass, mineral matter under these conditions and without regard to rate of motion may replace certain components in the wall-rock. This chemical interchange may take place along the walls of the channel or along the lines of the stratification-planes, jointing, or faulting. Whether mural or stratal in character there seem to be practical reasons for assuming that in the vadose zone cavity-filling and wall-replacement are partly, at least, direct functions of the rate of groundwater motion — the first prevailing when the current movement is slow, and the second taking place mayhap usually when the groundwaters are rapid and free-flowing.

The necessary consequences of a theoretical consideration of circulatory groundwaters have never been very critically compared with the recorded observations on the distribution of ore-materials in mines. Of course part of the waters moving horizontally in the vadose zone often issues at the surface of the ground as springs and becomes mingled with the surface-waters. At groundwater-level, or slightly below, the portion passing downward into the profound region at once drops a large proportion of its metallic salts to form the bonanza ore-zone. The part carried farther downward mainly enters into combinations which are not commonly ore-forming. Except at the top of the profound section of such channels few or no ore-bodies are deposited by these currents. When these downward-moving waters begin to ascend again, under ordinary circumstances, they are probably practically free from the common metals in solution; and on

account of these very conditions they doubtless do not take up metals on their way up to the surface of the ground again. The non-soluble character imposed during the passage of metallic substances through the profound region evidently remains so under all ordinary increases of temperature which they are likely to encounter.

On the other hand ascending juvenile waters appear to part with all or nearly all of their ore-forming materials long before they reach the normal groundwater-line and the confines of the vadose zone. The *locus* of principal ore-deposition appears to be near the bottom of the main channels through which the vapors and waters find exit from the magmatic masses. Only long afterwards when the ore-bodies are brought to the surface of the ground through the general erosion and lowering of up-raised belts of the country are they made accessible to man.

Since in the one case there is notable rock-solution and in the other rock cementation it follows that the ore forming tendency of the trunk-channels of the vadose zone and those of the profound zone are directly opposed. In the light of the most recent tests regarding the *locus* of maximum ore-deposition the theory of trunk-channel localization of ores, as advanced by Van Hise and others, needs to undergo considerable modification before it can be made acceptable. If under normally wet-climate conditions the greater part of vadose trunk-channels must be regarded as lines of ore-depletion rather than of ore-enrichment, the subject assumes added interest when it is considered that in arid tracts of the globe the vadose zone, as recently shown, attains enormous thicknesses and acquires vast importance in mining.

EDITORIAL

LAST MESSAGE OF BRANNER

On a morning a short month ago ye editor opened his mail to find a cordial letter from Dr. John Casper Branner. This epistle from the President Emeritus of Stanford University had joyous tone and small hint was there of impending tragedy. Its main theme was implied in the proffer of help in bringing closer together geological interests in the Western Hemisphere.

Having himself resided so long on the southern continent and possessing intimate knowledge of its scientific possibilities such as few men enjoy, the idea of a geological Pan-Americanism manifestly made strong appeal. With natural catholicity of sympathies which always characterized his activities in scientific work, he was not slow to see the opportunity to further an effort of which for long he was a leading exponent among earth-students.

Turning from letters to daily news ye editor on that fateful morning read first item that for the nonce took away his breath. It was announcement that Doctor Branner had departed this life the day before. Was there ever more sudden shock? That Pan-American letter was last message of the noble minded savant to this confrères before passing over the Great Divide. It was indited evidently with the hand of Death already closed upon his throat. The message reads:

Stanford University, Feb. 22.

Dear Keyes:

Someone has kindly sent me a circular about the Pan-American Geologist. I infer from the new name that it is proposed to have other countries of the continent join the effort, if proper associate editors can be found.

My acquaintance with the South American geologists enables me to suggest these:

Brazil: Horace E. Williams, Servico Geologico, Ministerio da Agricultura, Rio de Janeiro. He is a native of Arkansas, about 50 years old, and has been working on the geology of Brazil since 1893. He is a pretty good writer and makes his own maps and drawings.

Uruguay: Karl Walther, Instituto de Agronomia, Montevideo, Uruguay. I have not met him personally, but he seems to be an active and trustworthy geologist, and he is the best one there. I think he is a German; but he has been in Uruguay many years.

Argentina: Ask Bailey Willis. I am confined to my room by illness, or I would see Willis myself.

Chile: The only man I know in Chile who is interested in geology is Count de Montessus de Ballore, and his interest lies chiefly in Seismology. Address, Servicio Sismologico, Santiago, Chile.

Peru: Dr. Carlós I. Lisson, Servicio Geologico, Lima. Marsters can tell you of him personally and otherwise.

Truly yours,

J. C. BRANNER.

As a phase of the broader political Pan-Americanism uniting peoples of the Western Hemisphere in kindred spirit and hearty goodwill, Branner manifestly grasped at once the basic intent and far-reaching scope of an inculcation of the sentiment into science. He intuitively seized upon the event to affix a fundamental interpretation of the project and to further so far as he could its real motive. That he should rise so nobly to the theme, from the very pillow of his death bed, to speed on the idea of larger brotherhood among men is marvelous attest of the deep-seated hold which the very thought already must have had upon him. It is high tribute to his scientific attainments that his very last effort was for the promotion of the broad, humanistic feature of his science.

The quick and practical American mind advances already beyond the point in estimating the possible significance of the Pan-American notion. It asks what further advantage may possibly be derived from this interesting and novel transaction as its natural sequence. The international casket having been opened by this delicate key, are there no other jewels to be found in it of greater value than mere acts of reciprocal and republican politeness? May we not also find there the pearl of enduring goodwill between the several peoples, the emerald of a sincere international friendship, and the sapphire of mutual peace and justice for the years to come?

It is as some old violin string that feels the master melody — and snaps.

PASSING OF MURCHISON'S SILURIA

When, nearly a hundred years ago, Sir Roderick Murchison resolved the chaotic and mystical Transition Group of rocks of England into orderly arranged component parts he performed for Geology perhaps the greatest service of his century. As originally delimited the Silurian System corresponded essentially to the higher eral division which we now generally denominate as the Paleozoic section. This was about the same sequence which James Hall, of Albany, a decade later, attempted to erect and sustain, although unsuccessfully, as the New York System.

Siluria as first proposed, proved to be too large a group of terranes. This defect even its author was quick to realize. Soon it was agreed that his colleague Sedgwick should detach a bottom part under the designation of Cambrian System; and that another co-worker Lonsdale should decapitate it and erect the Devonian System. At a later date, Lapworth and other English geologists found it convenient to recognize the restricted Lower Silurian under a new title of Ordovician System. Thus, there was finally left only a minor part of Murchison's original Silurian section under the terminology which he gave it.

At the sight of Murchison's great geological edifice falling to ruins Giekie was led to bemoan, with not a little feeling, the loss of England's old geological landmarks: "Murchison's 'Lower Silurian' has by many writers been replaced by 'Ordovician,' and his 'Upper Silurian' is in a similar manner being ousted by some other terms, so that, if this process of substitution is perpetuated, the names given by the illustrious author of the 'Silurian System' will disappear from current geological literature. I shall continue to employ Murchison's terminology, which has the claims of priority, and, in my opinion, is perfectly sufficient for the requirements of science."

But petulancy and exchange of petty personalities are not scientific or logical arguments. If Siluria no longer harmonize with the advancing science there should really be small regret at its substitution, especially when new conceptions are involved. Had Murchison postponed the differentiation and naming of these

mystical Transition rocks geological classification and stratigraphic progress might have remained no farther advanced today than it was a hundred years ago. Siluria was launched without waiting for final word, fully discussed in all its phases for that early day, and finally, after long and useful career, gave way in the natural course of events, to more refined concepts of terranal grouping and delimitation. Its proposal and wide use in the first half of the last century were most appropriate, but it long since served its every purpose.

The basic purpose of Siluria was to give something of a time measure to older geologic sedimentation. When a more accurate standard became available the older measure had to give way to the newer. The ideal in this respect is, of course, some such unit as the year, or the century, in human affairs. This can hardly be in geologic computation; but the nearest approach to some distinctive unit is a desideratum. Now Siluria is not a periodic division by any means, but a division of higher taxonomic rank, one including a number of periods. As such it should have a time-span as nearly proportional to those of the other divisions of the same rank. This it does not seem to have either in its original signification or in its later restricted sense.

Bearing directly upon this phase of the Silurian question are some recent estimations on the length of geological time, which the several periodic divisions represent according to an adjustment of the processes of organic evolution. The majority of the recognized periods seem to respond to an evaluation of about 25 millions of years. The Siluric, or Upper Silurian, and the Devonian periods appear to have only about one-half of these values. Since the faunas of these divisions are closely related, and are with difficulty separated, and since together they hold to all intents and purposes a time equivalent equal to every other period it becomes almost obligatory to readjust the terranes to meet these conditions. Such a section could be known as the Yorkic, or Yorkian, Period, in commemoration of what is perhaps its most complete terranal representation in the world—the New York section of these rocks. Those who prefer may still use the terms Devonian and Silurian as alternate titles for the upper and lower parts of the rock section of this period. But Yorkic more nearly gives the

period its proper taxonomic value according to actual chronological standards.

UNVEILING OF THE CALVIN PORTRAIT

The Iowa Academy of Sciences recently did a most graceful act. It presented to the State of Iowa a life-size portrait of the late Professor Samuel Calvin. In initiating this practice the Academy entered upon a new and untried mission which is worthy of wide emulation. It was a happy thought to thus remember its most distinguished Fellow. A great State in doing homage to the name and fame of its nation-renowned scientist thus expressed its judgment of the true place he should hold among her sons who have done her greatest honor.

Iowa in accepting such memorial to hang upon the walls of the great art gallery of the Commonwealth's Hall of History takes on generous and enlightened aspect. It not only recognizes officially intellectual achievement but she ranges scientists along side of her other great men — the pioneer personages who founded the State, her statesmen, her executives, her public men, her generals, her clergymen, and her *littérateurs*. In the Historical Building there now hang the portraits of Charles Wachsmuth, Frank Springer and Samuel Calvin — geologists of whom the State may well be proud, towering as they do even among her most illustrious sons. This public and official recognition of high scientific endeavor is most fitting. Posterity will acclaim the selection and pronounce its benediction.

The portrait is the work of Iowa's most distinguished artist, Charles A. Cumming; and all who have viewed the picture audibly give expressions of realization of the consummate skill with which the painting is executed.

In the presentation of the Calvin portrait, Professor Thomas H. Macbride remarked:

Unable to appear in person for the present program, I have been urged to present myself in some brief address. This I assure you is accomplished not without difficulty. In such an attempt one misses so very much, if but in prospect, the happy concourse and sympathy of one's friends, the inspiration of fond, familiar scenes.

However, in the present instance, the task is lightened very much by virtue of the theme, and in the very purpose of our present simple,

though unusual ceremony. Sad reminiscence, from fountains however full, may from this hour and presence be repressed, the significance of our whole proceeding so easily, so readily a matter of felicitation.

As your committeeman, then, I beg to bring congratulations. This for several reasons. In the first place I venture to declare, as my settled judgment, that in the portrait before us, simply as a picture, we are indeed singularly fortunate. To be sure, in such a matter, each must form opinion for himself, but I expect for the days to come increasing compliment as the portrait becomes more and more familiar, not to members of the Academy only, but to observers generally.

I think it will be conceded now that our distinguished artist has given his subject careful and conscientious study; he has brought to our service long and patient labor, and a skilful brush. There were serious difficulties. Not only had the artist not known the subject of his effort, he had never *even seen him!* When we think of this, and reflect that for very form and inspiration, he had in all his work, naught but a few very ordinary photo-prints, the result is indeed surprising; not in artistic excellency alone but in accuracy and impressiveness. Our artist should also share the congratulations of this day.

In the second place, we may now rejoice in the Academy's intent and action, manifest so long ago; in the effort of the members and fellows to bring the plans of the Academy to fitting consummation, as in the program of this hour; in this we have, I am sure, a sense of satisfaction to be renewed, we hope, again and again hereafter as the years go by.

It is fitting and beautiful for colleagues and fellow-laborers in any field to put in pleasing form some recognition of service rendered, some indication of esteem, some memorial by which the past may, for at least a little time, enrich and cheer the future. Especially is this the case where, as in pure research, service is so often without personal emolument, without thought of gain or even cost, brought forward as a pure gift to humanity, promoted by the simple love of truth, devotion to the beauty, the order, and high significance of the physical world. And if, as now, the unselfish labor has been conspicuous, the vision brilliant, the attainment great, the work accomplished memorable,—do we not honor ourselves in thus handing over to the State of Iowa, for the men and women of tomorrow, and yet tomorrow, memorial such as this? Lo! here some concept of Iowa's most devoted lover; of the master-student of her prairies, her rivers, her forest, her flowers, her rocks, her soils, nor less her wonderful far-fetched history locked indeed in the very form and structure of soil and stone, but revealed, set forth, not to this Academy alone, decade after decade, but to the young men and women of the Commonwealth assembled in scores and hundreds as class succeeded class in the great University; and at last, to the scientific world, in volumes—today the grace and pride of the science of the State.

Here is no place for history or biographic details, did one dare divulge it; but may I so far abuse my privilege, and your patience as to tell how fifty years ago, and for many continuous years thereafter I saw a man go forth; in an open wagon, sometimes borrowed, more often hired, sometimes his own, traversing the road-less, bridge-less prairies of northern Iowa; enduring the heat of August suns, chilled by the damps of night, shelterless, tortured by mosquitoes, drenched by wild thunderstorms that made terrible the midnight hours; breakfasting at dawn and toiling until his campfire burned beneath the evening star. From Lansing to Clarinda, from Dubuque to Mason City, to Winterset, to Ottumwa; athwart the State, through the State, around the State he moved; climbing all rocky heights of nature's carving, pondering the talus of every open quarry, every wall of crumbling rock or sliding shale, wading the creek-beds and tracing the banks of larger streams, away from home for weeks together;—I knew such a man; in such fashion, and not otherwise, did he win the rich experience and world-wisdom presently brought in such overflowing measure to the State of Iowa!

Not for what it has cost, but for what it means, we commit now to the keeping of the public, this simple memorial of our colleague. His work is finished, but shall abide long as men live who love their heritage of time; may the work of our artist long endure!

In other words and centuries a people, reputed still the wisest, wittiest of earth, not only discovered that 'art is long' but likewise also seemed to know that only the skill of the artist does in some mysterious way avail to transmit the soul of things, the thing called inspiration to future days and centuries. So they took care of art. They saw to it that men in after times should see what form had Pericles and Plato. Blind Homer nor less Socrates found memorial in marble, if reports are true. Not all were equal to their greatest, but under their greatest every Greek could claim, did claim, and flush with pride that thrills even to this day.

Sometime, perhaps where the social work and institutions of this Commonwealth of ours shall have become from river to river more homogeneous, shall crystallize as such things do, when the migration of people shall cease,—sometime this, our people, shall perhaps appreciate their own, sometime mayhap a 'temple of fame' shall rise. Shall it be in some vast physical structure with marble columns shining, shall it be in some noble masterpiece of letters, than brass or marble more enduring, lit by the light of intellect, by passing centuries unworn, undimmed? Whatever, whenever, or wherever the memorial rise, of this let us be sure,—upon it the name of our colleague shall appear, among the first have place, and all other commonwealths may rival us if they can!

Nay, my colleagues, there shall still remain, for all those whose names in honor shine, memorial nobler, more enduring far. The State, the State itself a living thing, into its fibre have passed the lives of all who thus at the beginning toiled to make it great! The State, sane, noble, intelligent, immortal as we hope, shall inevitably bear in its very character

the thought, the purpose high of these its founders, memorial long-lasting as the course of time.

On behalf of the Academy, in moving the acceptance of the memorial, Prof. Bohumil Shimek spoke feelingly:

Rising to move the acceptance of the beautiful gift here presented, I do so with much hesitancy, for two reasons.

It was not until a little while ago that I learned just what was expected of me on this occasion, and there has been no time for even the orderly arrangement of the thoughts which should here find expression.

Then, too, I fear that the flood of memories which will come all unbidden will make it hard to do justice to the memory of the man whose kindly face looks out upon us from the canvass here presented.

I first learned to know Professor Calvin more than forty years ago, when, as a Freshman, I entered his department as the factotum whose duty it was to furnish field-supplies for laboratory work, and during all the years that followed my respect and affection for him grew constantly. He was both teacher and friend, and it is difficult to decide in which capacity he gained the stronger hold upon the affections of those who were brought in close contact with him.

Neither time nor the occasion will warrant an extensive account of Professor Calvin's activities. As already noted in the deeply sympathetic letter of his long-time friend and colleague, Doctor Macbride, this is no time for biographical detail. We recall with pride his services as a citizen and a soldier; his scientific achievements are a matter of record never to be forgotten; and the memory of his splendid character will remain longest with those who knew him best.

It is nearly fifty years ago that he came to the State University as Professor of Natural Science, and the record of his life is blended with the history of the development of the University and the State. Out of the chair of "Natural Science," or "settee" as he facetiously called it, have grown the strong departments of Geology, Botany and Zoology in the College of Liberal Arts, and that of Bacteriology in the College of Medicine. He was the organizer of the Iowa Geological Survey and for many years the State Geologist, and the record of his work in this connection is too well known to require repetition in this presence.

While we cannot dwell upon the details of Professor Calvin's life, there are two qualities that stand out particularly as characteristic of him as a teacher, an investigator and a man, which seem to be especially worthy of note at this time. I refer to his extreme modesty and his sterling honesty. Would that it were possible to burn the memory and the appreciation for the value of these qualities into the minds and the consciousness of the younger generation of scientific workers! A man of strong convictions, yet he approached every problem modestly and with an open mind. There was none of that air of cock-suredness which is sometimes displayed by the narrow specialist, and which is sure

to arouse mistrust. No doubt this modest attitude largely prepared the way for the soundness of his conclusions when finally reached.

His modesty was but a phase of that honesty which was his transcendent quality. He was not only honest in ordinary dealings, but he was honest with himself, and honest in his attitude toward the scientific problems which engrossed his attention. It is this phase of his character which I commend especially to those who are just entering upon a scientific career, for there is no other field in which open-minded honesty is more truly essential.

May this beautiful gift assist in perpetuating the memory of our beloved friend and colleague, and may the example of his noble life inspire us, and those who follow us, to an honest search for truth!

Accepting the painting on part of the State, Curator Edgar R. Harlan, of the Historical Department, made the following brief but appropriate address:

A function of the Historical Department of Iowa is to have at hand the facts and the materials which testify to the merits of Iowa men and events. Merit so proved, which remains permanently apparent throughout all time, is the object of all true effort of the scholarly and the inspiration of all, unless the selfish, of every calling. The selection and preservation of the proofs of merit and of attainment being the duty of the office I for the time occupy, it has been a constant, deep and firm satisfaction with which I have received the knowledge today and in other days of the great place arrived at by Samuel Calvin. The position led to by him, of the science, or branch of science, of which he was the chief Iowa ornament, as by your unanimous voice today I am advised, is a place respected through all the realm of scientific thought in America.

Not many types of evidence bear more sure and satisfactory testimony to the character of a man than this well done portrait. Carlyle has taught us best of that. And this canvas done honestly, considered now by you finally, presented formally to your State, shall carry with it to the place of its perpetual deposit, the stamp of your approval and thereby the indisputable claim of value as a work of history as it is a work of art.

When a man has risen well up toward the ideal of life and its attainment, he is become the symbol of his own day and a standard for succeeding generations. He is the exceptional man who in his early life sees clearly the lines within which he is to choose his work. He is a courageous character who discerns that these lines do not converge in some single aim and yet who contemplates their courses and their distance in his objective. Science deceives no follower through rainbows or vanishing points in life's perspective. He is a blessing to mankind who early sees and boldly abandons all to the purpose of advancing across and beyond known confines into regions where the timid would, but dare not, go. He who thus advances blazes new paths in the wilderness,

sets up permanent landmarks, and rears to himself in the realm of mentality lasting monuments.

Iowa seeks to note and to preserve the proofs of such of her noblest lives. Something it is she does, but all too seldom and too imperfectly. But in this service of your Academy of Science amends, in part, are made.

In the domains of science, unlike in some other fields, men rise to great distinction, expand, achieve, and yet impinge on none. They tower above without obscuring those with whom they stand. They expand without crowding or absorbing others in their expansion. They achieve as brother fellows in achievement. They initiate, they stimulate, they direct; they do not subtract, nor repress, nor deflect force. Samuel Calvin was such a man. He saw early and with clearness. He wrought with certainty and with prodigious dilligence. He went afar, and expanded always. He achieved at the expense of no other man's achievements. He found a world that was made larger by the extent of his expansion.

In leaving behind him such a memory such a man leaves also opportunity for the succeeding generation to do honor to itself in honoring him. In your endeavor to affix his lineaments for posterity you make highest effort men can make to transmit to those who shall ever be his beneficiaries some token of his appearance in life. And in that long future throughout which men shall use his thought and be blessed by his accomplishment they will pay their respects to this work of art as they do to his works of science.

Clothed with the custodianship of such priceless treasures of the State of Iowa it is mine to thank the Academy, mine to accept the custody, and mine to pledge the people of the State to its propriate care and disposition; mine to express to you a gratitude profound for the privilege of being associated in an enterprise so noble and so enduring as are the benefits which the public will have forever flowing from the great labors of this distinguished man.

One regrettable incident entered into the otherwise felicitous exercises. Neither the geological department of the State University of Iowa nor the Iowa Geological Survey, to which were devotedly given up long years and best efforts of Calvin's life, were adequately represented, and the Survey had no representation at all. Such oversight passeth all understanding. The *gaucherie* was widely observed and by not a few freely commented upon.

STRATIGRAPHICAL GEOLOGY

Scope of Cretacic Sedimentation in Andean Geosyncline. A recent preliminary contribution to the Cretacic paleontology of the Andean geosyncline by Fritzsche¹ lists ten localities, extending from Yaco, Bolivia, to northwestern Argentina, where fossils have been found in the so-called Puca Sandstone of Steinmann.

These collections are especially interesting since a number of them contain assemblages indicative of brackish and fresh-water conditions. A similar brackish and fresh-water fauna was obtained by Singewald and Berry at the Finca El Molino, in the valley of the Rio Tarapaya, about nine miles northwest of Potosí, and 5 miles southwest of Miraflores, where the same Cretacic beds also contain marine fossils.

The El Molino fauna comprising myriads of small Cyrenas and Ostracod tests, and a sparing representation of Melanoides, Cerithium, and Planorbis occurs in thin limestone beds intercalated in the gypsiferous red-beds so typical of the Puca Sandstone. Fritzsche's comparisons are with European Early Cretacic brackish faunas, especially forms described by Dunker from German lagoonal deposits of that age; and he concludes that the age of the South American beds is Barremian, and that they are to be correlated with the coal-bearing Cretacic beds of Peru. That the Puca Sandstone is not Early Cretacic in age or the same age as the Peruvian coals is the reason for publishing the present note.

In the first place the nature of the material does not warrant the placing of much reliance on either real or fancied resemblance to European fresh- or brackish-water forms, which are in general notoriously unreliable. In the second place some of the forms such as *Hadraxon* and *Melanoides* are scarcely normal in Early Cretacic assemblages. In the third place, at the locality near

¹ Centrallblatt f. Min., Geol. & Pal., 1921, No. 9, pp. 272-277, 1921.

Miraflores, about 5 miles down the valley from El Molino, a sandy limestone at approximately the same horizon, contains a limited marine fauna. Steinmann's collections from the former locality contain, according to Fritzsche, *Pseudodiadema rotulare*, Desor, *Holectypus* sp?, *Lima* cf. *galloprovincialis*, Matheron, and *Nerinea* sp? (The most abundant form in our collection from Miraflores is the form that Fritzsche calls *Pseudodiadema rotulare*, Desor. We have about one hundred individuals of this form and these are identical in the structure of their ambulacral plates, the crowding of the pores near the peristomial margin, the solid spines, and imperforate primary tubercles, with a species of *Cyphosoma* common in the Late Cretacic rocks of Peru where it is associated with a varied and typical fauna. It would be singular that we should have collected from a limited outcrop, scores of *Cyphosoma*, whereas Steinmann collected only *Pseudodiadema*. The genera are superficially similar and the conclusion seems warranted that both names refer to the same Echinoid. Moreover they are associated with sparse remains of a *Pecten* close to the so-called *P. quinquecostata*.

This fixes the age of this part of the Puca Sandstone as Late Cretacic and not Barremian age, and leads to the suggestion that the gypsiferous, lagoonal red-beds, or Puca Sandstone, instead of being of the same age as the littoral and continental coal-bearing Cretacic deposits of Peru, is really contemporaneous with the maximum extension of the Late Cretacic sea in the region occupied by the Andean geosyncline, and represents the shoreward deposits along the eastern margin of the geosyncline over an area 500 to 600 miles in length, lying east of the present divide of the Eastern Andes, which were contemporaneous with the Late Cretacic limestones represented from southern Peru northward to Colombia, and southward in Chile and Patagonia.

The exact stage in the Late Cretacic Period of this maximum expanse of the Late Cretacic sea is not yet definitely determined, nor does it seem possible to reach final conclusion with the present knowledge of the faunas; it may be early as the Cenomanian or as late as the Emscherian, but it is certainly not either earlier or later.

BERRY.

Yorkic Period of Stratigraphy. In a recent recalculation, adjustment, and evaluation *de novo* of the periodic time-units so that the span of all subdivisions of geologic chronology of this rank may be subequal, the Devonian and Silurian (Upper) divisions, as commonly recognized, proved altogether too short. The first of these was only about two-fifths, and the second only about three-fifths, of a normal unit. Together their time-span equaled that of other periodic divisions.

Use of the title *Yorkic* in this connection is a recognition of the circumstance that in New York state is displayed the most complete development of the (Upper) Silurian and Devonian sections in the world. It is not in any sense an adaptation of the early New York System of Emmons, Mather, Vanuxem and Hall, in 1842 and 1843, because the limits of their section were coterminous with that vast rock-pile now recognized as spanning the Paleozoic era. Delimitation lines of the major subdivisions of the New York System are approximately those of the European classification which was already properly defined when the New York proposal first appeared in print.

In view of the fact that the Murchison *Siluria* embraced virtually the entire Paleozoic section, that he, himself helped to detach a considerable upper portion under the title of Devonian System, that Sedgwick removed a lower part under the term of Cambrian System, and that Lapworth closed the bitter Murchison-Sedgwick controversy by suggesting the name Ordovician System for the disputed middle section — the Lower Silurian of the one and the Upper Cambrian of the other — there is only left of the original Murchisonian succession the Upper Silurian rocks which constitute a quite subordinate portion of the whole.

And now when the time values of sedimentation are alone to be considered, or measured, the last remnant of *Siluria* merges into a later adjudication. Rather than attempt to extend Murchison's title so as to include the Devonian section where the latter originally reposed, or to restrict the original definition, it appears to serve better the canons of modern geologic nomenclature to drop the old names and adopt an altogether new title, remove the typical section to the locality where involved rocks find best development,

and allow elastic usage of old terms, until gradually without controversy, they sink into innocuous desuetude.

KEYES.

Derivation of the Peter Sandstone. Because of the remarkable purity of the sandrock, the small size of the components, and the peculiarly perfect rounding of the grains the Peter Sandstone is often regarded as having been formed by eolian means. Despite these features the formation is now demonstrated to have been laid down in the sea. Marine fossils are found abundantly in some localities. One place alone furnishes no less than fourteen genera, embracing 13 species of pelecypods, 7 species of gastropods, 3 cephalopods, 3 brachiopods, 1 bryozan and 1 sponge.

Although in the north the entire Peter section is massive sandstone, in the south there are important limestone beds intercalated.

The history of the components of a sandstone is complex. It takes into account transportation successively by winds, streams and waves. Textural criteria alone afford uncertain proofs of the exact nature of the transportation even to the latest deposit in which the sand-grains are found. Complexity of this history is further increased when the sand passes through several cycles of migration, from loose beds to solid rock and then through loose sediment to solid rock again.

Compared with average marine sandstones, in content of clay, iron, mica, heavy minerals, and carbonates, the purity of the Peter sandstone does not depart sufficiently from that of associated marine sandstones to demand any essentially different explanation of origin. The degree of purity actually existing, as determined by special chemical analyses, is satisfactorily accounted for by assuming a derivation from older already well-sorted sandstones.

After weighing the evidence on the various possible sources of the materials composing the Peter sandstone, the Pre-Cambrian occurrences of the Texas Llano region, of the Rocky Mountains, and of the Ouachita Mountains, all seem to be out of the question. This leaves only the Canadian Shield in the north to consider. At the time the Peter formation was laid down this land-mass embraced not only the Pre-Cambrian crystallines, but also broad belts of so-called Potsdam sandstones of Cambrian age. This for-

mation, already composed of well-rounded components, well-sorted materials, and reduced sandgrains, readily supplied inexhaustible quantities of characteristic sands which are delivered to the sea by rivers and perhaps in part by the winds.

C. L. DAKE.

Complexity of Peter Sandstone. When Owen, in 1852, named one of the heavy sandstone formations exposed to the Rivière Saint Pierre, now only known as the Minnesota River, it was believed that it was a compact unit with no differentiable parts. This was the subsequent opinion of all who worked in the upper Mississippi Valley for many years afterward. But the notable unconformity at the base of the section, as observed in later years, clearly indicated that there was history involved other than any yet noted.

When the Ozark region came in for special attention little attempt was ever made to closely parallel any of the lower sandstones with the northern Peter sandstone. In the early nineties of the last century the impression gained credence that the First Sandstone of the early reports of Missouri which had sometimes been denominated the St. Peter sandstone, was not really the typical development of the formation in the north, but something more. For this reason, mainly, the title Cap-au-Grès Sandstone was used for the nearest Missouri occurrences, in order to recognize the distinction. It was then also found that calcareous beds were intercalated in the sandstone section and that only the part below the limestone horizon could be probably connected with the typical Peter formation of Minnesota.

Just what the exact stratigraphical equivalency was was not then very clear. However, it was certain that the original St. Peter section was not a simple lithologic unit, but several. Hence, in plotting the general geological section of Iowa Peter sandstones were raised to serial rank. The absent sedimentation, represented by the unconformity at the base, was thought to cover the Canadian interval of the East.

Under the title of Minnesotan Series the sandstone succession thus acquired higher taxonomic rank, although in Iowa it embraced apparently a single formation. In the meanwhile special terms designated the several terranes in the serial succession

around the Ozarks. Everton Formation, including also the median limestone layers, was used in Arkansas for the lower part. As a basal sandstone this portion of the sequence was seemingly the only part represented by sediments in the north, in Iowa, Minnesota and Wisconsin. Probably the Arkansas name should now be restricted somewhat, and be made to designate only the limestone; and a new term proposed for the sandstones.

The early correlations which are sometimes made between the Ozark and Lake Superior sections have to be taken with a great deal of caution. In new cycles of sedimentation special conditions have to be analyzed, the exact terranal equivalencies are not only uncertain, even when the lithologic similarity of sequence seems identical, but are especially misleading when long distances are not open for inspection and direct tracing from point to point in the field is not possible.

The provincial Minnesotan Series of the Mississippi Valley therefore embraces (1) a basal Peter sandstone, (2) a median Everton limestone, and (3) a superior yet unnamed sandstone for which perhaps the Cap-au-Grès should be retained. The inferior unconformity merits closest scrutiny throughout the entire region from Lake Superior to Texas.

KEYES.

Extension of Triassic Coal-field of North Carolina. Important coal deposits of Triassic age prove to be more extensive than had hitherto been suspected. This coal-field known as the Deep River Basin lies mainly in Moore, Chatham and Lee counties. The entire northwestern side of the Triassic basin is now known to be underlaid by a commercial coal-bed 40 to 50 inches in thickness. In that portion of the seam which was opened up in the vicinity of Farmville and Cummock and which was extensively tested, there are, according to M. R. Campbell, not less than 15,000,000 tons of excellent steam and coking coal; and the best estimates for the remainder of the field are at least 60,000,000 tons available within depths of 1500 feet.

Coal outcroppings are traceable for a distance of twelve miles. By means of drill-holes and shafts the coal is now proven into the center of the basin for a distance of one and a half to three and a half miles, at which line the seam passes below a depth of

1500 feet. On the northwest side of the field the coal-bed dips at angles of 20 to 25 degrees; but within a few hundred feet of depth there is a general flattening towards the middle of the basin. At Farmville the dip along the axis of the basin is less than 5 degrees. Several narrow dikes of igneous rock cut the coal-bed at various points. These are likely to cause some slight trouble and extra expense in mining operations. Beyond the dikes the coal is again encountered in undisturbed condition. A few faults of small throw are present; but these are confined mainly to the margins of the basin.

As shown by the following analyses the ash content of the coal is low. The sample is from the mine of the Carolina Coal Co., near Farmville, being taken from the face of the first left entry, 75 feet from the foot of the slope.

Analyses of Triassic Coal

PROXIMATE ANALYSIS		ULTIMATE ANALYSIS	
Moisture	1.8	Hydrogen	5.2
Volatile matter	32.5	Carbon	77.1
Fixed carbon	58.8	Nitrogen	2.1
Ash	6.9	Oxygen	6.3
		Sulphur	2.4
		Ash	6.9
Total	100.0	Total	100.0

Calories, 7716.

British thermal units, 13890.

Analyst, H. H. Cooper.

The mining conditions in the Deep River district are, on the whole, excellent. The roof is a thick bed of hard sandstone underlain by 2 to 10 inches of "draw slate." The latter usually does not fall until some little time after the coal has been removed. The floor, which is composed of "black band," offers an excellent hard foundation for the car tracks. As the coal-bed carries but very little water, the entries, galleries and rooms are comparatively dry. In the Cumnock mine, which is close to Deep River, the rooms and entries are so dry that it is necessary to sprinkle them in order to keep down the dust. It is necessary to use safety lamps as the coal gives off considerable gas but there should be but little trouble to maintain a proper ventilation.

At the present time 1500 feet is considered the limit to which the coal bed can be profitably worked; but conditions may arise that will permit of deeper mining, and in that case there will be a considerably larger tonnage than that given above.

Since this coal-field lies over 175 miles east of the nearest Appalachian coal-fields it is that much nearer to the markets of central and eastern North Carolina. Two railroads, the Southern and the Norfolk-Southern, cross the field; and the Seaboard Air Line Railway is within seven miles of the center of the field.

J. H. PRATT.

Muscogee Shales of Western Interior Coal-field. In Kansas, Missouri and Iowa the nethermost member of the productive coal measures is widely known under the designation of the Cherokee Shales. The title Cherokee is Haworth and Kirk's. Proposed by these two writers many years ago the name first comes into general geologic usage in 1894.¹ Without any particular effort towards bibliographic research bearing upon the availability of the term or determining the validity of such proposal the name passes into rather general usage.

Unfortunately, it now transpires, the Haworth name as a formational title proves to have been long since pre-occupied. So early as 1869 W. C. Kerr,² state geologist of North Carolina, affixed the same name to certain slates, or shales, in his state. This circumstance leaves the Kansas terrane apparently with opportunity for acquiring a brand new name. However, there happens to be a title which with some slight shifting and redefinition virtually spans the same vertical section and which with unimportant modification in present signification may be made to take the place of the Kansan Cherokee. This is the Oklahoma term Muscogee.

The Muscogee section, as originally defined by Gould,³ is almost the exact equivalent of the earlier proposed Arkansan Series,⁴ the proposal of which occurred a decade before for this very thickening of the coal measures lying south of the Ozark

¹ Kansas Univ. Quart., Vol. II, p. 105, 1894.

² North Carolina Geol. Surv., Rept. 1866-7, p. 29, 1869.

³ Research Bull., Oklahoma State Univ., No. 3, p. 3, 1910.

⁴ Proc. Iowa Acad. Sci., Vol. VII, p. 128, 1901.

uplift. But the original Muscogee formation also embraces the Cherokee Shales of Kansas and Missouri. It is not at all probable that the shales to which Cherokee was applied will ever be classified as a part of the Arkansan Series, for strong diastrophic reasons. Thus treated the Muscogee becomes an unusable and useless term. Whether it could ever be restricted and redefined so as to take the place of Cherokee is perhaps somewhat questionable. Yet it might be, without doing serious violence to the canons of nomenclature. There is, however, already another and later title in the field, one which is virtually co-extensive with the Kansas term. This is Vinita Formation, proposed by Sieben-thal,⁵ and afterwards further described and defined by Ohern.⁶ But the same objections and also some others obtain concerning Vinita as they do with Muscogee, so in this respect there is little to choose between the two.

Rather than have to deal with the complications which an entirely new title might introduce it seems probable that the title Muscogee should be best fitted to the situation. By elimination of the great lower Arkansan Series, which has clear diastrophic definition, it leaves the Oklahoman term applicable to its remaining upper portion, which is an exact equivalent of the old Cherokee shales, now nameless. This solution of a difficult problem in nomenclature is doubtless the only one by which the term Muscogee may be retained as a valid terranal title.

KEYES.

Wide Extent of Texas Potash Formations. Late discoveries of potash minerals in the Permian red-beds of western Texas promise large developments along this line, perhaps the most important in the entire country. These red-beds contain, as is widely known, extensive deposits of common salt; and it is associated with these that the potash occurs.

Texas salt beds on the High Plains appear to have been deposited from the concentration of sea-water in narrow arms of the ocean through a long period toward the close of Paleozoic times. In the course of the desiccation of these waters potash salts in the brines are precipitated last whenever the points of

⁵ Bull. U. S. G. S., No. 340, p. 191, 1908.

⁶ Research Bull., Oklahoma State Univ., No. 4, p. 12, 1910.

saturation for these salts are reached. Evidently desiccation did not go far enough in many cases to precipitate the potash. It is scarcely to be expected that evaporation of the waters in such arms of the sea should continue through a long period of time without resulting in the laying down of a considerable number of salt beds and that desiccation should in every case have stopped short of precipitation of the potash. In the many cases of partial or total desiccation indicated by the number of salt beds found in the Permian section, it is more likely that the waters were in some cases completely evaporated. Reasoning in this way, there is good ground for the belief that potash should exist in separate beds in that part of the Permian basin where most common salt was precipitated. Finding of potash salts in no less than seven wells, two of which are located in the Panhandle and five in the Llano Estacado, strongly tends to prove the correctness of the conclusion that widespread beds of potash exist in this region; but the observations that have so far been made give little or no information as to the thickness of the potash-bearing beds. All we know is that there are such beds. To determine thickness of the beds in order to find whether these deposits will prove of commercial importance, it will be necessary to drill special holes for that purpose and to take out cores of the beds.

The potash-bearing mineral is, in almost every case noted, a red salt, which seems to be ordinary polyhalite. This mineral normally contains about 18 or 19 per cent of potash. Its occurrence in sufficient quantity in drillings taken at considerable depths makes it appear desirable especially to investigate the magnitude of the deposits. This can be done only by coring the salt beds. As the salt and potash crystal are both soluble in water, the taking of cores through such beds requires special and expensive machinery, and to obtain the results desired it must be done under competent scientific and technical direction. The latest five places where potash has been found are about 60 miles apart. It seems possible that the potash-bearing strata may extend the entire distance between these places and possibly up to the Panhandle. From a recent inspection by N. H. Darton, it appears that conditions that may be considered favorable for the natural concentration of salt and also potash in the Permian

formations in which these salt beds occur, may have been widespread, reaching possibly from western Oklahoma southwestward to the Pecos River. The largest part of this area lies in Texas.

UDDEN.

Mid Ordovician Volcanic Ash in Tennessee. A deposit of volcanic ash, known as bentonite, was recently disclosed in the grading of the Dixie Highway east of Shelbyville. This bed, according to E. S. Larsen, is a decomposed rhyolitic ash, somewhat akin to leverrierite which has been greatly altered immediately after eruption.

It occurs in the top of the Carters formation (Lowville), of the Mid Ordovician section. This stratum is 21 inches thick; and is divided into three distinct layers. At the top of the bentonite is a half-inch streak of white, hydrated calcium carbonate; then 10 inches of green bentonite, with streaks of white hydrated calcium carbonate extending through it in nearly vertical lines; then five inches of yellowish, sandy bentonite, containing rounded grains of quartz, black mica and feldspar; and a basal layer of five inches of green bentonite, resting upon the smooth surface of dark blue, crystalline limestone. The junctures between the three layers of bentonite bed are very regular; but the contact between the top of the bentonite and the overlying limestone is irregular, and, in places, this dove-colored, thinly bedded limestone cuts down to within one or two inches of the top of the second layer of the bentonite bed.

Other samples of bentonite have also been obtained from well-cuttings in Pickett and Overton Counties, and from McMinnville, in Warren County. An outcrop of the bed at Pruetts cut, near Watertown, in Wilson County, was recently described by the writer, that was thought to be bentonite, but no good samples could be obtained for tests.

Ulrich mentions a bed of clay occurring at the horizon of the Carters formation, at High Bridge, on the Kentucky River, in Kentucky, which recent investigations show to be this same bentonite bed. At Birmingham, Alabama, a layer of bentonite, which is probably the same as the Tennessee layer, was found within 30 feet of the top of the Ordovician succession, in the bottom of the shafts of the Tennessee Coal, Iron and Railroad Company.

These widespread occurrences of a bed of volcanic ash, or bentonite, in the Ordovician strata of Tennessee and the neighboring states, indicate that additional discoveries along this line may be looked for throughout the Appalachian region of eastern United States.

NELSON.

*Galena Limestone as a Terranal Title.*¹ In the name Galena Limestone there lurks a last, lingering trace of Eighteenth Century nomenclature. It is, perhaps, the only instance of the kind which remains in this country. During a long period of more than two generations the title, so manifestly invalid as it appears to be, unscathingly runs the entire gauntlet of terminological criticism. How it manages to hold its own despite the vicissitudes of all these years is something of a mystery. Although plainly a Wernerian relic it appears today almost as valid a geological title as any of its latest geographic compeers.

Galena Limestone does not appear, as is generally surmised, to be a specific place name that was at first applied to a special rock formation. Strictly speaking the term does not refer to a definitely defined terrane at all. As originally proposed the designation really covers merely an irregular, mineral-bearing portion of a regular stratigraphic unit. Only recently is the title extended to a terranal sense.

When the term Galena Limestone was first suggested by James Hall,² so long ago as 1858, it was intended to particularize the dolomitic, lead-bearing body of what had been previously known as the Upper Magnesian rock. Thus, at the time when the title Galena was first used geologically, it alluded strictly to the ore-content of the formation. Although at this period he had already begun the practice of proposing geographic names for geological terranes in New York state Hall had not yet entirely emancipated himself from Old World influences of his youth, or of his Wernerian teachers and contemporaries in the East.

Of the twenty-five terranal titles which Hall used in his Iowa reports fully one-third of this number are old mineralogical names. Moreover, the rock terms in common usage in the mining

¹ Published without permission of the State Geologist.

² Geology of Iowa, Vol. I, p. 60, 1858.

regions were fresh in his mind. Schoolcraft³ many years before had used "Metalliferous Limestone," and Featherstonaugh⁴ had described in some detail the "Galeniferous Limestone." Hall's proposal was clearly a third name having identically the same mineralogical significance.

It is sometimes taken for granted that Hall's Galena Limestone derives its title from the country east of Joe Daviess County, Illinois, which was at that time a center of lead mining industry of Illinois. There is nothing in the original description to support this notion. J. D. Whitney, who was Hall's chief assistant on the Iowa Geological Survey, plainly indicates that there was no intimate connection between the two terms. His nearest allusion is the following statement:⁵ "The custom house at Galena, a city surrounded by bluffs of the Galena, is built of rock from the Carboniferous Limestone group."

As a geologic title Galena Limestone was proposed under misapprehension of its true stratigraphic affinities. At that time the lead-bearing dolomite was believed to be a distinct stratum, reposing normally upon the blue fossiliferous limestone. This notion generally prevailed until very recently, when Prof. W. H. Norton⁶ made the important observation that the real difference between the two alleged terranes was merely local and lithologic and not general and formational.

With Norton's clue as a basis Professors Calvin and Bain particularly investigated the so-called Galena-Trenton rocks of Dubuque County the center of the lead field. They found several distinct life-zones passing on the same level from the normal, well-known "Trenton" limestone through the altered "Galena" dolomite. This circumstance alone proved beyond peradventure that the two lithologically distinct rock masses really constituted one and the same terranal unit.⁷ Singularly enough in their table of formations for Dubuque County, the authors mentioned adhere to the old conception,⁸ placing the Galena unit above the

³ Narrative Journal of Travels through Northwest Region of United States to Sources of Mississippi River, etc., in 1820, Albany, 1821.

⁴ Rept. Geol. Recon. to Coteau des Prairie, 159 pp., Washington, 1836.

⁵ Geology of Iowa, Vol. I, p. 290, 1858.

⁶ Iowa Geol. Surv., Vol. VI, p. 146, 1897.

⁷ Iowa Geol. Surv., Vol. X, p. 409, 1900.

⁸ *Ibid.*, p. 398.

Trenton stratum. In his last word on this subject Calvin⁹ considers the Dubuque section alone; and designates all beds, whether mineralized dolomites or normal barren limestones, which lie between the blue, Maquoketa and the green, Decorah shales as Galena Limestone. This happens to be very nearly Hall's position in the beginning.

Nowhere, therefore, in all the geological literature relating to this region does there appear to be a single instance in which the title Galena Limestone is used in a strictly geographic sense, with a definite type-section noted, and a distinctive fauna delimited. The original mineralogical significance undimmed, persists to the present day.

In view of the fact that several terranal units are now clearly differentiated from the old Galena-Trenton formation, that a distinct formational unit is established which embraces both normal blue layers and their altered dolomitic phases, and that according to modern canons of geological nomenclature such stratigraphic units should be fixed by geographic titles, it appears necessary, notwithstanding its long usage, to discard the old mineralogic term Galena as a terranal name. It is therefore proposed to apply to the formation lying immediately above the Green, or Decorah, shales a geographic title derived from some Dubuque locality where the terrane is displayed in typical section. Julian Limestone is suggested — from the township in which the city of Julian Dubuque is situated, and where at the famous Eagle Point the most accessible exposure is characteristically shown.

In their relations to the dolomite and the normal limestone the life-zones of the Julian formation are particularly instructive. Some of them are incidentally recognized by Calvin. The Green, or Decorah, shales underlying the terrane under consideration are especially characterized by an Orthid fauna. Two species in particular, *Orthis subquadrata*, Conrad, and *O. tricenaria*, Conrad, are prolific in their occurrence. One hundred and sixty feet above the base of the formation is a zone 10 feet in thickness which is unusually rich in gasteropods. Thirty feet higher is another 10-foot zone which is composed largely of rhizopod remains — the gigantic *Receptaculites*. Both of these prominent

⁹ *Ibid.*, Vol. XVII, p. 192, 1907.

life-zones, as well as others less conspicuous, pass without slightest interruption from the unaltered limestone to the dolomite. These relations are best indicated by diagram as represented below (Fig. 18).

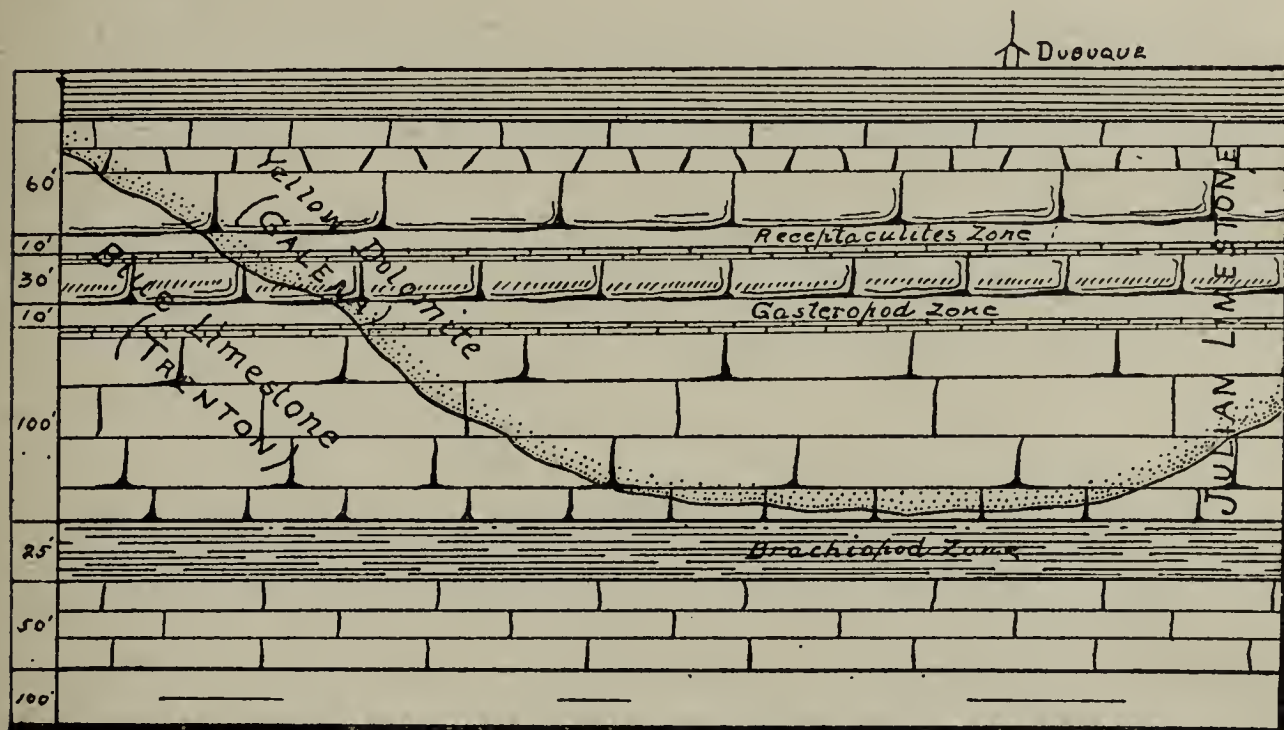


Figure 18. Relations of Dolomite and Normal Limestone.

Hall was not, as is commonly supposed, the first worker to determine the faunal position of the Ordovician section in Iowa. When the author of the Galena Limestone arrived on the ground in 1855, he found that the Blue unaltered lime rock of the Dubuque region had, by J. N. Nicollet¹⁰ already been paralleled, on the basis of its contained fossils, with the Trenton group of New York. Likewise D. D. Owen¹¹ had recognized in the Iowa rocks organic remains which he regarded as characterizing the Trenton horizon of the East.

Late Cretacic Formations in English Channel. The existence of Late Cretacic rocks in the central deep of the English Channel was recently ascertained from the results of the dredgings of the "Pourquoi Pas?"

Flint nodules, entirely analogous to the flint nodules of the Chalk of the Paris Basin, cover in very great quantity the whole bottom of the central depression of the English Channel. The

¹⁰ Rept. Intended to Illustrate Map of Hydrographic Basin of Upper Mississippi River; Sen. Doc., 26 Cong., 2nd Sess., Vol. V, pt. ii, No. 237, Washington, 1841.

¹¹ Rept. Geol. Surv. Wisconsin, Iowa and Minnesota, 638 pp., Philadelphia, 1852.

collected specimens show there an important outcropping of the Late Cretacic beds with this chalky facies, extending along the axis of the Channel; and prove that the opening of La Mauche Channel already existed at that epoch, which allowed communication between the Cretacic seas of the Paris Basin and those of the Pyrenees and the Mediterranean regions.

This is the first time that the geologic study of the bottom of the sea off the French coast has been undertaken systematically with the aid of a new contrivance used by the Commander.¹ The same apparatus was used again last year (1921) during the second cruise of the *Pourquoi Pas?*

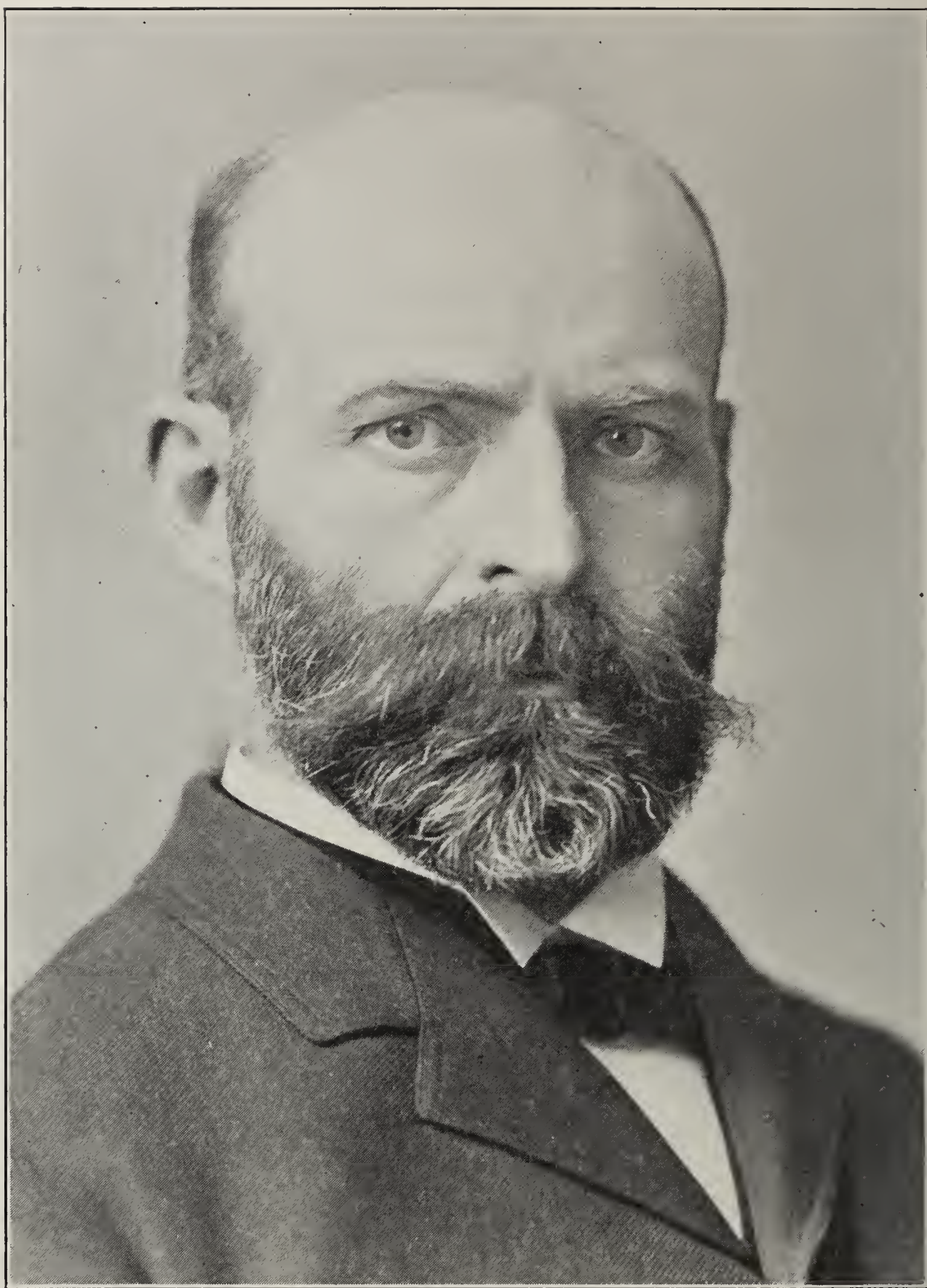
CHARCOT.

Dakotan Sandstone in Missouri. There was recently obtained by members of the Missouri Geological Survey, and sent to me by Director Buehler, a large sample of dark brown, or chocolate-colored, coarse-grained sandrock which had been collected in Mercer County, a few miles west of Anderson, near the northern boundary of the State. It was at once recognized that this sample was a piece of the typical Nishnabotona, or Dakotan, sandstone.

Although the possible presence of Cretacic strata in north Missouri had been long suspected this is the first real indication that rocks of this age actually exist there. Judging from the submitted sample alone there was no information showing that it might not be merely a fragment of a Drift boulder. However, in later inspection of the locality, a mass of similar sandstone was disclosed in such position and of such size as to preclude recent transportation.

By singular coincidence the locality is such that a very considerable outlier of the Dakotan sandstone should be preserved. It is very close to the line of the great Cap-au-Grès fault, which here has a displacement of about 100 feet. Being on the down-throw side and thus depressed below the general plains-level, the Cretacic beds, which undoubtedly once widely covered this region, are preserved from regional planation. Thus on the same level, flat-lying Cretacic strata but flat lying Carbonic beds.

¹ Doctor Charcot was the commander of the *Pourquoi Pas?* during the dredging operations of both the first cruise and the second cruise last year, 1921. — ED.



J. C. Branner

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JOHN CASPER BRANNER

BY CHARLES KEYES

Disciples of the Master who have drunk
The vigorous draught from his deep well of thought
Surround the earth. And they have felt his fire
Along their limbs. His power has made them strong
To conquer things almost unconquerable.
His words have gone with them into far lands
Beyond the borders of the seven seas.
And they have borne his teachings to the youth
Of later generations. Men his eyes
Have not beheld are strengthened by his strength.
They strip their work of devious ornament
In honor of his stern simplicity,
And know him as a prophet.

No mean sphere
The teacher fills, who has the might to bridge
The distances in time and space, as he.
His power can find no limit while his words
Live in the hearts of many earnest men.

So, in the hearts of many men, he lives.
But we who loved him know the void he left
Can not be filled. His genial smile, the glints
Of deathless humor in his kindly eyes,
His able hands, that children loved to clutch
And cling to, and his great untiring brain
That had its benediction in hard work
And recompense in work sublimely done —
These things are vanished into yesterday.
Today is gray, since the Master passed.

DOROTHY GUNNELL JENKINS.

Through the recent demise of John Casper Branner American Geology loses a unique figure belonging to two continents. The great Stanford University misses one of its most distinguished scholars, one of its greatest teachers, and one of its most respected and beloved personalities, its President Emeritus.

Doctor Branner was born seventy-one years ago, in New Market, Tennessee, on July 4, 1850, and died at Palo Alto, California, on March 1, 1922. His father was Michael T. Branner; and his mother before her marriage was Elsie Baker. In 1883 he was married to Susan D. Kennedy, of Oneida, New York. Their children were John K., George C., and Elsie, Mrs. W. F. Fowler.

His early education was obtained in the schools of Dandridge, Tennessee, where he attended Maury Academy; and then he entered Maryville College. Later, at the age of eighteen years, he enrolled at Cornell University, from which he was graduated with the degree of Bachelor of Science in 1874. At Cornell University he fell in with Charles F. Hartt, David Starr Jordan, and others of a congenial Cornell coterie all members of which afterwards became famous.

Before completing his college course at Cornell young Branner was selected by Professor Hartt, who was acting as Imperial Geologist of Brazil, to assist on the geological survey of that country. Associated with him was also Orestes St. John, who, with Hartt, had previously visited South America in connection with the celebrated Thayer Expedition to Brazil, under the guidance of Louis Agassiz. Among other student assistants who went out on the Hartt survey were Orville A. Derby, Richard Rathburn, and Herbert H. Smith. This work occupied several years and prevented young Branner from finishing his work at Cornell until 1883. Upon the death of Professor Hartt, in 1875, Branner became Director of the Imperial Geological Commission. In after years Branner was repeatedly called to the Brazilian field. In 1882 he was commissioned by the United States Government to visit South America to study insects injurious to the cotton plant and the sugar cane. When Brazil became a republic he entered the service of the Sao Cyriaco Mining Company, at Minas Geraes, assuming the duties of engineer and interpreter.

Branner also visited Brazil and Argentina in the capacity of botanist for Thomas A. Edison, searching for woods especially

fitted for certain electrical uses. Before getting back to the United States he represented for a short time our Department of Agriculture in the first mentioned state. But his work in South America was not yet finished. In subsequent years he directed at different times no less than three scientific expeditions to Brazil. One of these, in 1899, was under the patronage of Alexander Agassiz. Another, in 1907, was financed by Dr. Richard A. F. Penrose. And a third, in 1911, was made under the auspices of the Brazilian Government. The last had for its immediate purpose the investigation of the geological and biological features of the sea-coast on either side of the mouth of the Amazon River, in order to determine the effects of the large volumes of fresh water brought into the ocean by this great stream, upon marine life.

Besides his extensive geological experiences in Brazil Branner was engaged for two years on the Geological Survey of Pennsylvania, where he mapped the rocks and relief features of the Lackawana Valley. For a period of five years he served as State Geologist of Arkansas. A score of volumes attested his great activities in this direction. Some of these reports were model and comprehensive monographs of their kind. Associated with him on these investigations were R. A. F. Penrose, Arthur Winslow, J. Francis Williams, Leon S. Griswold, R. Ellsworth Call, Thomas C. Hopkins, Gilbert D. Harris, and Frederick W. Simmons. His own results were mainly contained in a half dozen bulky volumes.

Although so long and so widely given up to geological investigation much of Doctor Branner's preëminently successful career was devoted to the teaching of geology. When yet on the Pennsylvania Geological Survey, in 1885, he received appointment to the chair of geology in Indiana State University, which post he held for six years, when he was called to the faculty of the then newly founded Stanford University in California, where the remainder of his useful life was spent. During fifteen years of this period he acted as Vice-President of that institution; and succeeded Doctor David Starr Jordan as President, retiring under the age limit established as Emeritus in 1917.

Concerning Doctor Branner's association with Stanford University, Chancellor Jordan feelingly writes: "My acquaintance

with Branner covers fifty-two years, the first two as fellow student and fraternity brother in Delta Upsilon, the next thirty as fellow-teacher and co-worker in science in Indiana and in California, three more as my successor and colleague in administration of the educational work to which I gave the best twenty-five years of my life, and, finally five years of retirement from active responsibility to the congenial work of writing out of the fullness of experience. In all these years he lived up to his motto, 'I can get along without the respect of my neighbors, but not without the respect of Number One.' And in maintaining self-respect, he won the regard of his neighbors of whatever degree. A righteous life helps to strengthen all who come in contact with it. 'There is always room for a man of force and he makes room for many.'

"I may say that I joined Delta Upsilon in 1870 because Branner and Comstock, two of my dearest friends, were already in it, and I couldn't afford to stay out of Heaven when they were looking over embrasured in its balcony."

President R. L. Wilbur adds: "Dr. John Casper Branner's outstanding characteristics were a love of order and truth, fidelity to every obligation, and a respect for the rights of others. He had the keenest sense of loyalty and of devotion.

"As the founder and head of the Department of Geology, at Stanford University, he showed rare foresight and executive ability. Much of his success was due to the fact that he gave a general course in Geology to all of the students in the University who desired to take the work, and that he insisted on giving the elementary work in his own way to the students electing Geology and Mining for their life work. He took the greatest interest in watching the development of his students, particularly in those who became associated with him in the educational field. Because of his insistent industry in his lifetime he was able to do an unusually large amount of scientific work. He was remarkable in his punctuality. It was a saying among the students that you could set your watch when you saw him pass from his home to his laboratory in the early morning.

"His long and careful studies made of Brazil deeply interested him in the problems of the whole South American continent. In the development of the Branner Geological Library, the work

of a lifetime, he also acquired a large collection of South American literature. His interests were almost cosmopolitan and world-wide. His influence through some of his former students and associates can be felt in all parts of the world today. We miss him greatly here at the University. In his later years he added a fine touch of wholesome and patriarchal dignity and sincerity to the life of the Stanford campus. During his illness the little children expressed the love for him that all felt."

Dr. J. M. Stillman notes: "As a teacher Professor Branner exerted upon his students an influence which inspired them to their best efforts. His broad experience, his own systematic and untiring research, his realization of the supreme importance of practical experience—as the final test of all theories, were well calculated to stimulate the ability and energy of his students, while his simple, sincere, and sympathetic personality attached them to him with a rare devotion. The distinguished careers of many of his students evidence the efficiency of his teaching. His ideals of the geologist's training were high. They demanded breadth of culture. In a wholly admirable address on the 'Training of a Geologist,' given before the Indiana Academy of Sciences, in 1889, he says: 'The man who goes into Geology because there is money in it, will, in nine cases out of ten, make a failure of it—he will get neither the money nor the geology. To be sure, a living must be had, but he who has the right training and the right interest in his work will never lack for lucrative employment for any considerable length of time. . . . The world is too full of problems of a scientific interest for any man having a scientific spirit to stand idle for a single day, or a single hour, and no one having such a spirit will stand idle.' Again he says: 'The man who has no notion of accepting the results of his reasoning would just as well not reason at all, while the man who undertakes to reason within certain limits insults his intelligence. All honest men are seeking the truth and is it not our duty to help others in this search when we can? We may be sure that if we wait till all the world thinks alike, the world will never care what we think.'

"These public addresses of Dr. Branner—none too many of them are published—contain much that is autobiographical, and always are expressive of the man himself, for I know no one

who spoke more directly and sincerely from his own experiences and personality.

“In an address at the Centennial of his boyhood’s school — Maury Academy at Dandridge, Tennessee — are passages such as these: ‘In those times we boys used to have codes of honor that were not without interest and not without their uses. These codes had some features that were foolish and childish enough, but they also had this which has probably not been improved upon: “A gentleman must honor women, and he must never lie, cheat, or steal.” I leave it to any fair-minded person to say whether or not boys who have such a code, and who try faithfully to live up to it, have in them the stuff out of which fine men ought to be made.”

“‘Whatever success I have made in this world I attribute to a desire that has always been strong in me to help everyone who needs my help.’ “Another statement I must ask you to accept on faith is that I never in my life gave or intend to give advice to others that I do not regard as being for the best interests of those to whom given.” And in this same address, pleading for free opportunities for education of the young men and women, he says:

“‘If you would have your sons and daughters honor and bless your memory after you are gone, help them up — up into the clearer light, where they can see and feel aright and appreciate fully the efforts you make for them. There will be your great reward. You cannot lead your children to think ill of you by educating them, but if you educate them not, they may feel justified in thinking ill of you, and if out of unworthy motives you fail to do your duty by your children, there is reserved for you a hell whose horrors have never yet been told.”

“A very dominant ideal of Doctor Branner was his concept and practice of loyalty. In an address to the students of Stanford in 1908, he took ‘Loyalty’ for his subject. It is a well formulated expression of the self-respecting and consistent loyalty that marked his relation to institutions and to associates. Space permits of but brief quotations:

“‘Without making any fine distinctions, I start with the proposition that loyalty is the most valuable attainment, if we may call it an attainment, or a most valuable trait of character, if that is a

better name, that any man or any people can have in this life. And I challenge any one who questions this theory to put the matter to any test he chooses to apply from the highest moral standards down to the lowest commercial ones."

" 'You may fairly ask what is to become of loyalty when the conditions make it impossible. One always has a remedy in his own hands; he can quit, and carry with him a gentleman's self-respect, for without that there can be no loyalty worthy of the name.'

" 'Remember too that loyalty, like charity, begins at home. When can one see a finer sight than that of a family that stands compactly together, helping and encouraging one another within, and defending each other from without.'

" 'Loyalty is one of the big far-reaching virtues; it makes trustworthy men and great men; as a national virtue it makes a people great. For if it is love that makes the world go round, it is loyalty that holds the world together.'

"In an address to the Stanford student body in September, 1905, full of excellent counsel, I can but cite two illustrations, as pertinent to these times as to that in which he was speaking.

" 'Whatever you may make your major subject, I want to commend to every one of you the daily use and cultivation of the English language. To that end speak the best English you can at all times. I would not have you a lot of affected prigs, but neither would I have you cultivate the conversational style of a Bowery tough.'

"In October, 1913, at a dinner given in San Francisco by the Stanford Alumni and friends in honor of the accession of Professor Branner to the Presidency of the University, it was my privilege to be the faculty representative speaker. In the course of my remarks I said: 'Therefore I may safely omit on this occasion any attempt at personal characterization and simply remind you that twenty-two years of loyal, devoted, and distinguished service to Stanford University, a life-time of high ideals, of integrity, and purity of life, of character without a cloud, entitle him to the loyal support of all Stanford men and women. You know him, we all know him, we know his strength and gentleness, his firmness and kindness, his courage, his patience, his sincerity, his earnestness, and his genial humor.' The years succeeding have

to me but added a deeper significance to the words then spoken. Doctor Branner's life is a great heritage to Stanford University, for California, and for the Nation."

Professor Branner's services to science were widely recognized. He was elected to membership to many learned societies. Among others, he belonged to the National Academy of Sciences, the American Philosophical Society, the American Association for the Advancement of Science, being its Vice-President in 1890, the American Geographical Society, of which he was president in 1900, the Geological Society of London, the Geological Society of Edinburgh, the Société géologique de France, the American Seismological Society, being its President in 1911, the Geological Society of America of which he was an original fellow, besides member of the several geographical and geological societies of the various Brazilian states.

Of academic degrees he received his B. S. from Cornell University in 1874; Ph.D. from the State University of Indiana in 1885; L. L. D. from the Arkansas University in 1887; also from Maryville College in 1909; and from California University in 1915; Sc. D. from Chicago University in 1916. The Academy of Sciences of Philadelphia conferred upon him, in 1911, the Hayden Medal "In recognition of the value of his contributions to geological science, and of the benefits derived from his able and conscientious discharge of the official trusts confided to him." Besides his early governmental commissions to South America he served as special United States Commissioner on the Panama Canal, and on the California Earthquake Commission in 1906.

Doctor Branner's geological publications covered wide scope. In addition to the more pretentious volumes on Brazilian and Arkansan geology there are innumerable articles and memoirs which appeared in the transactions of the learned societies and in the geological journals. His publications related mainly to science applied, the culminating effort being perhaps the "Syllabus of a Course of Lectures on Economic Geology," written in collaboration with Professor J. F. Newsom. This is one of the best outlines on the subject ever printed. For conciseness, clearness of statement, and logical arrangement this volume probably has no rival.

Of his many addresses few found their way into print. Fewer

still related to geology. Yet his masterly presentation on the "Training of a Geologist" stands today as one of the American classics. Who ever stated the prerequisites of a successful geological career more truly and concisely than he in his presidential address before the Indiana Academy of Science thirty years ago. "The general academic training of a geologist during the first two or three years of his college course is not essentially different from that of any other man of culture. I am not disposed to side with those who think that if a man is to be a specialist the sooner he begins his specialty the better. In a general way this proposition is correct, but in making such an admission it must be distinctly understood that all things which tend to broaden a man's scholarship form essential parts of his specialty. That a man should have a knowledge of history, philosophy, social science, and of literature in general goes without saying. But as bearing directly upon his professional career he should understand of the languages, at least the Latin, French, and German. In mathematics he should have the general instruction required by civil and mining engineers, and should give special attention to astronomy and geodesy. In chemistry the more thorough his training the better; and beside the usual work required of students of chemistry, in which especial attention should be given to inorganic chemistry, he should be a skilled mineralogist and should be well acquainted with metallurgical processes. In physics the student should give attention to optics, especially as employed in the construction of mathematical instruments; to hydraulics and hydrostatics, to dynamics and to hypsometry.

"Besides having a broad general culture, a geologist must be *par excellence* a geologist, and besides being a mere geologist he ought to know more about some particular branch of geology than any one else. The material progress of our times is due largely to the division of labor which enables each individual to perfect his skill. Progress in science is due in no small degree to a similar division in scientific work. Though I cannot dispense with a knowledge of chemistry, specialization by a neighbor who devotes himself to chemistry relieves me of the necessity of devoting a large part of my time to chemistry; the devotion of another to physics gives me my time for geologic work proper, which, without the specialist in physics I should be obliged to devote to phys-

ical studies. The astronomer hands me the results of his special investigations and saves me my time for geology, which, without his help, I should be obliged to give to astronomy. And so it is all around. On the other hand I trust that my attention to geology will, in its turn, come to the aid of the chemist, the physicist, and the astronomer."

Professor Branner's sympathies were preëminently catholic. They were never narrowed down to a single line alone. His interests in botany, in entomology, and other branches of natural science were varied and broad. Nor did he stop with the sciences. Among his more notable excursions into pure literature may be mentioned his grammar of the Portuguese language which grew out of his Brazilian experiences. His "Bibliography of Clays and Ceramics," an important compilation; the "How and Why Stories," a charming collection of southern negro dialect myths; his genealogy of "Casper Branner of Virginia and His Descendants;" and his recently completed, but as yet unpublished, translation from the Portuguese, of Alexandre Herculano's Establishment of the Inquisition in Portugal, all evidence his breadth of interests and his tireless energy.

Doctor Branner was a man of imposing appearance, fine address and pleasing manner. Tall, with notably robust physique, and well proportioned, he possessed commanding presence. With ceaseless effort he pursued his investigations. Indeed, it was this very superabundance of energy and great persistence of effort that led him to overexertion among the hills of Arkansas, where he had gone a year before his death to finish some of his early work, injuring his heart so that he was invalided for the rest of his life. A man of sterling character, he was distinguished in the class-room for his dry humor, unfailing readiness, and good nature. As a teacher he was exceptionally successful in directing young men to thorough and accurate dealing with the intricate problems of earth study. It is, perhaps, from the angle of the teacher rather than of the original investigator that his great services to his chosen science should be evaluated. In him the two fields are combined in quite remarkable way. In neither domain are his efforts likely to be soon forgotten.

EARTH'S FUTURE MIRROR'D ON FACE OF MARS

BY PROF. GEORGE H. HAMILTON

Lowell Observatory, Flagstaf, Arizona

Our knowledge of the Earth in its past, present and future states has been acquired through exploration and the interest such exploration has invoked in its devotees. At first glance "an explorer" calls to mind one who travels the Earth's surface and gives us knowledge of its topography; the boundaries of its seas and lands, its mountain ranges, valleys and plains, and the faunas and the floras found there. Exploration of this kind gives us knowledge of the Earth's *present*.

But what of Earth's *past*? Geology must answer that.

To explore the Earth's depths, and to find therein evidence of a gradual evolution of its crust, an upbuilding through the ages from the time of its first formation in the concrete to the present time, has a fascination all its own. The means at hand to do this, in themselves show an evolution from the primitive and increase the certainty that the Universe as a whole is bound by inexorable laws of cause and effect — *omnia mutantur et nos mutamur in illis*.

The explorer became the geologist. Gazing on a mighty escarpment and seeing the various strata thus exposed, with its folds and faults plainly in view, forced him to reason out the causes of such stratification and the probable time expired in their laying down. In general, erosion gives him only a superficial knowledge of the crust, but in the case of the Grand Canyon of Arizona thousands of feet of the surface have been worn away down to the primitive igneous rocks, giving us a history so old that man's mind really fails to grasp it.

And this is not all; there is evidence of erosion on a vast scale down to the present rim of the Canyon. Strata upon strata have vanished never to return again. This is known from remnants

still left, vast buttes standing alone on the plains which in other regions far removed from the Canyon's rim are themselves deep below the surface of the existing ground.

Where erosion is lacking, it is now possible for the geologist to drill deep into the Earth's crust and thus encounter the various strata, exploring now to depths that it would have been impossible to reach even a few years ago. In this manner is the history of the Earth unfolded.

What of the other worlds that are now known to be scattered in space, sharing with the Earth the status of blocks in the mighty edifice of the Universe? Reaching back only to the formative stage in its life history when the crust of each world was in process of making, we know, at present, only a very few. These are the planets which, with the Earth, form the Solar System. Jupiter, the largest, has had, and will have, a prolonged period in which to write its own history. In the telescope Jupiter is a densely cloud-covered sphere, through which can be dimly seen what is suspected as the ruddy glow of its seething and molten surface. Owing to its large size and mass it has taken very long to cool. Its atmosphere is still composed, not only of the gasses known on Earth, but of water-vapor, which will at some time condense into seas, and many other substances that are now in a solid state upon the Earth.

The planet which most nearly approaches the Earth in the similarity of its surface is Mars. Smaller than the Earth — only 4200 miles in diameter — its atmosphere is such that one is able to see through to the surface below and any change there recorded passes under our eyes. It is the province of the planetary astronomer to interpret the changes seen and by long continual observations not only perfect his knowledge of those data, but also, by considering the causes which have produced these effects, delve deeper into the past history of the planet and that which goes to form it.

The geological significance of the formations on the planet Mars is gone into very fully and in a most interesting manner by Percival Lowell in "Mars as the Abode of Life." His chapter on "The Genesis of a World," explains in a delightful way, the terrain there seen.

It appears that, being smaller than the Earth, Mars has never had as much water in proportion to its surface — though in pro-

portion to its mass it may have had. Having grown and agglomerated as the Earth in sweeping through space and also being a near neighbour to the Earth, it very probably consists of the same materials; though being of smaller mass, it is less dense and may never have attained to that temperature known to be necessary in the formation of the heavier materials in the body of the Earth.

Seas, at present, it has none; though we have evidence of areas which denote sea-bottoms. If we were to reverse the proportion of seas to land on Earth we would come near to the proportions that once held sway on Mars. Its vast ochre areas which are seemingly now deserts once might have been called continents — as a matter of fact it is the term used in speaking of them now. The large dark areas, of far less extent, are considered to have once been oceans, but on account of their periodic (seasonal) changes, with all the characteristics of such changes on Earth, they are now considered areas of vegetation. Their changes in extent and colouring with all the tones seen on Earth as the seasons follow each other plainly tell us that this is so.

As one wanders over the Earth one is struck by the large deserts belting the globe above and below the equator; and by studying them one is led to believe that the Earth was not always so, nor will be. That the deserts are gradually encroaching on the cultivated areas is patent. During historic times ports that were once flourishing are now ruins far inland; showing that the seas themselves are gradually drying up. Aqueducts in various portions of the world now betoken a civilization long dead. Where once these same aqueducts watered a fertile land the desert has intervened and of water there is no sign.

It is of interest that these desert regions, where water has once held sway, are comparatively flat. Water has done its work. Erosion, except for that of the winds, is complete. Where there now is water there is still erosion; it seems to be a gradual and timed proceeding, or as one might put it, there seems to be just enough water on the Earth to denude and flatten it.

The Earth as it is said, found its level as a spheroid of rotation; the unevennesses of its surface still remaining will be gradually smoothed off by wind, river and ocean. A fine example of this is seen in the San Francisco peaks of Arizona. The contour of the vast crater now remaining shows by its shape that it was once,

before the volcanic upheaval, a mighty cone far higher than it is now, and that since the crater formed it has been cut down to its present height, and that it is still lowering.

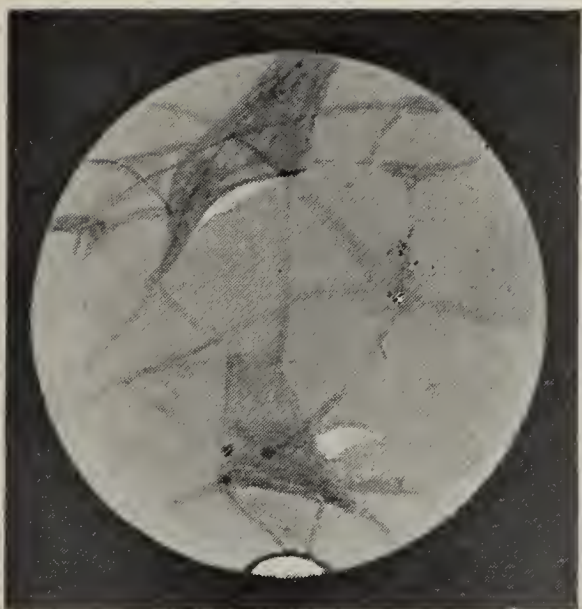
This opens up a vista in the far-distant future that, to our minds, denotes death to the world and all upon it. The picture is not as awe-inspiring as it seems. The sea-beds will be the last to become desert, vegetation will still flourish there when the continents as we now know them will be parched, bare and lifeless.

Mars, as we see it now, has reached this stage in its history; what were once continents, lapped by inland seas, now are deserts; the seas themselves now tracts of verdure, full of life as yet, as is evidenced by the changes seen occurring there. Its surface is comparatively even, a height of two or three thousand feet above the surrounding regions could be detected with the telescope—either as a projection on the terminator of the planet, or from the shadows cast by the rising or setting sun. Nothing of the sort is visible.

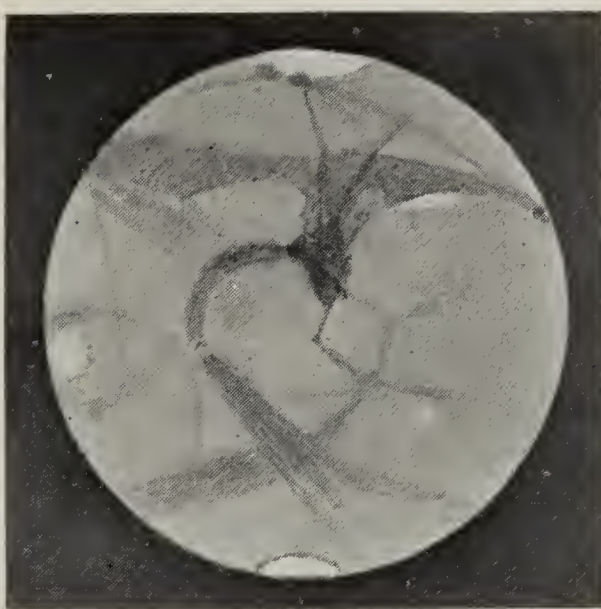
What mountains there may have been on Mars have long since been eroded, and the lack of water to the extent of that known on Earth has been no obstacle; since the smallness of the planet, its internal energy and contraction through cooling, has all contributed to lesser altitudes in any mountain chains or folds in the Martian crust that may have appeared.

Erosion is seemingly a preparation for this geologic era that all planets must at some time in their history pass through. Mars would today be in sorry plight if its surface were as rugged as that of the Earth. As the seas dry up, so it seems does the atmosphere of a planet. What were once plains watered by copious rains now are deserts; the rains ceasing not only on these areas but over the whole surface of the planet as well. How then does moisture reach the vegetation which we know exists in the dead sea-bottoms?

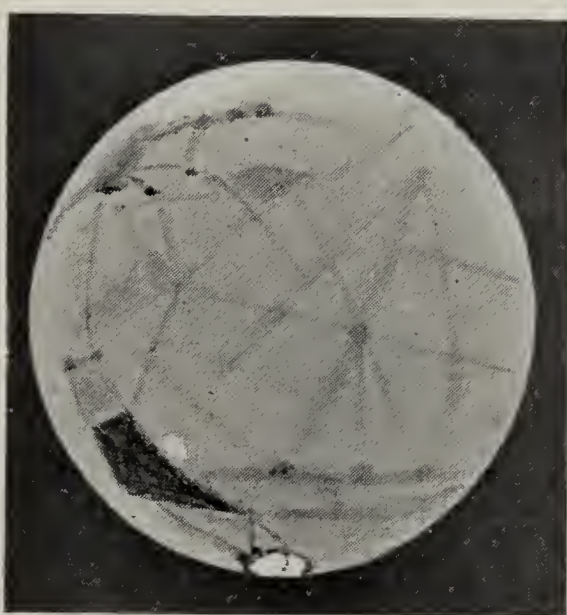
An interesting fact comes on the horns of this dilemma and, as we see it on Mars at least, smooths out what once was rough. In the ordering of Nature, since life came upon the Earth, fauna and flora have been inseparable. From the time of their differentiation evolution in the two groups has progressed side by side—and it seems that evolution is ever upward despite the talk of some people of “devolution.”



APRIL 6



APRIL 26



MAY 11

MAY 12



MAY 22



SEASONAL CHANGES ON FACE OF MARS

Since we have every evidence of vegetation on Mars, there may be animal and intelligent life as well. If so, this life must have fitted itself for the ever changing climate and topography of the planet. For animal life to exist it must preserve the vegetation upon which, from all analogy to things earthly, it lives. Over the vast plains of Mars comes the water from the melting polar-caps, the only portions of the planet's surface upon which at present the waters can condense. This water seems to be drawn from the poles along the level surface of the planet to the areas of vegetation by artificial means. It is here that the far-famed canals of Schiaparelli and Lowell come into play and about which much has been written elsewhere.

As the water leaves the poles, it only shows itself to the eye and the telescope by the tracts of verdure along its course — and these tracts appear as canals. This vegetation, in turn, announces itself by its characteristic changes similar to that seen each season in the large dark areas. The vegetation along the canals waits upon the melting of the polar-caps each spring and the large areas again wait upon the water as it progresses downward from the poles through the canals. The level surface of Mars enables that to be accomplished which would otherwise seemingly be impossible.

The canals reach up to and into the polar-caps. As the caps melt each spring and the snow becomes thin it quickens the vegetation below it. This in turn melts the snow above the canals and rifts appear in the caps. These rifts prove themselves to be the canals by remaining *in situ* as canals, after the caps have receded sufficiently to leave them bare.

The vegetation proves itself vegetation in this case also, since it is dependent on moisture for its growth as on Earth, also from the fact that in the act of growing it liberates sufficient heat to melt the snows above it *before* the snow on either side is dissipated by the solar rays.

The past history of our Earth is gradually unfolding. The present is known moderately well. Knowledge of the future remains to be unraveled. The revelation may be hastened by the study of our sister planet out in space.

HIPPURITES FROM SOUTH AMERICA ¹

BY PROF. EDWARD W. BERRY

Johns Hopkins University, Baltimore

It is an appropriate commentary on the lack of geological knowledge of the greater part of South America that the recent Williams Expedition should have obtained but two specimens of fossils from the Bolivian altaplanicie between Lake Titicaca and La Paz, and that each of these forms should represent a different Late Cretacic marine type new to South America, and from a region where no Cretacic rocks have ever been recognized, a tract moreover, more traveled than any other that could be mentioned in that general region.

The first of these types, a new species of the typical Late Cretacic echinoid genus *Echinocorys*, was described in a recent note. It was picked up by natives at Peñas, about 56 km. northwest of La Paz, and about 10 km. from the eastern shore of the Laguna Uinamarca — the southern and small division of Lake Titicaca, from which it is almost entirely severed by the peninsulas of Copacabana and Hachacache.

The second specimen was also collected by natives and came from detrital material in the valley of the Rio Milluni, a small stream taking its origin in the Devonian tract of the southern slopes of Huayna Potosi and flowing into the Rio Vilahaque 12 km. northwest of La Paz. I am indebted to Señor Arturo Poznansky, the director of the Instituto Tihuanacu for this valuable specimen. The latter owes its preservation to the fact that it is encased in an argillaceous, marcasite-impregnated concretion. It shows the surface features of three individuals of the genus *Hippurites*. The material displays the attached valves; and although these are not sufficient for complete diagnosis, the form is undoubt-

¹ George Huntington Williams Memorial Publications, No. 11.

edly new, and one that may be readily recognized by collectors from the accompanying illustration, the work of G. S. Barkentin. The species may be named

Hippurites boliviensis, n. sp.

Attached valves broadly conical, with slightly convex profiles; low, the largest about 4 centimeters high and 6 centimeters in maximum diameter; elliptical in marginal outline, the minimum diameter one centimeter less than the maximum diameter; furrows faintly indicated in the two larger forms, but slightly more prominent in the younger form; columnar areas not different in ornamentation from the rest of the surface; ornamentation a combination of transverse, somewhat irregular laminae and upright folds, which are faint at the base and become more pronounced upward; inner layer of the shell, internal features and cover valves not seen.

The species shows more or less resemblance to a number of south European and north African species of Turonian, Santonian and Campanian ages, but in the absence of actual specimens for comparison there is little to be gained by a discussion of these resemblances derived from a study of illustrations, and besides it is believed that there is little of age significance in such resemblances. The Bolivian form appears to be clearly referable to the genus *Hippurites* of the family Hippuritidae, although it shows some resemblance to certain species of the genus *Sphærulites* of the allied family Radiolitidae, which lacks the smooth bands of the genus *Radiolites*.

The only previous record of the occurrence of the sub-order or super-family Rudistacea in South America appears to be that by Fritzsche,² who records *Agria Blumenbachi* St. from a number of localities in Peru and Chile.³ This he considers to be of Urgonian (Barremian) age, from the resemblance to rocks supposed to be of that age in East Africa and Persia. *Agria* is probably a *Sphærulites*, and hence belongs to the family Radiolitidae. Associated with *Agria* Fritzsche records what he calls *Requienia ammonia*, Goldfuss, which he seems to consider a Rudistid, although it belongs to the less specialized alliance of Chamacea.

² Centralb. f. Geol. Min. & Pal., 1921, No. 9, p. 276, 1921.

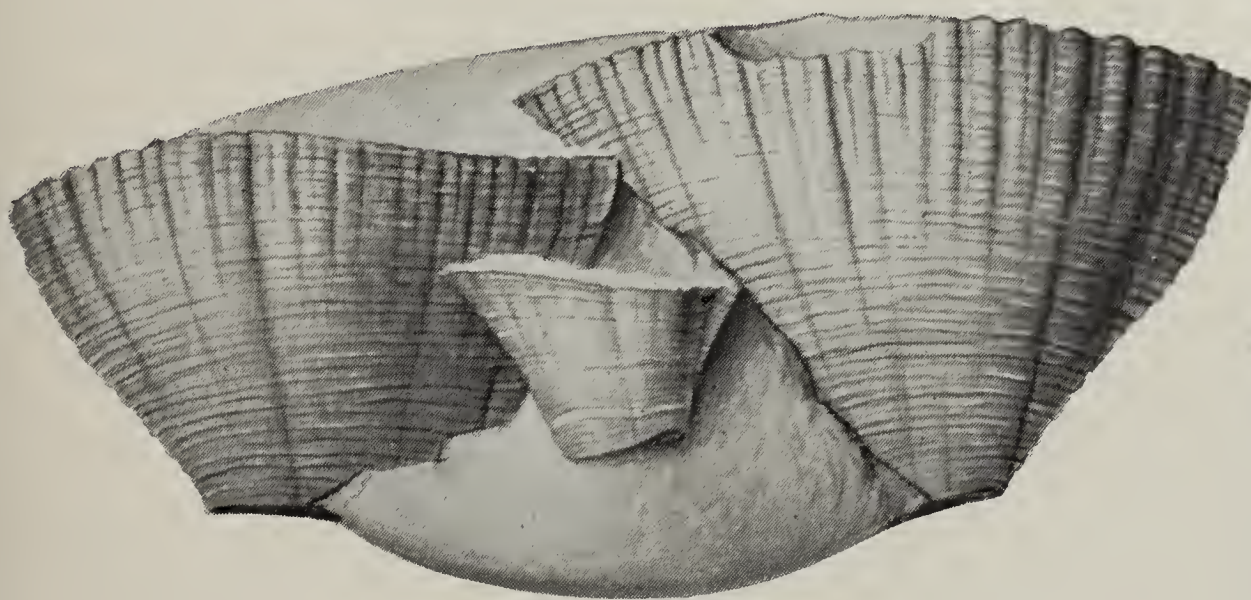
³ These localities are Matash near Huallanca, and Acopampa, near Huaraz, in Peru; Potrero in the Copiapo Valley, Paso Malo, near Arqueros (ast of La Serena) and between Arqueros and Rodaito, in Chile.

D'Orbiguy,⁴ in 1847, figured a very imperfect specimen from the Cretacic rocks of Chile, which he called Hippurites (?), but it is so indefinite that it has not been considered by later students.

The exact age of *Hippurites boliviensis*, or whether the containing formation still outcrops along the eastern margin of the Bolivian altaplanicie, is unknown. The form is evidently a Late Cretacic type, and probably is of the same age as *Echinocorys poznański*, Berry, from the same general region. I regard both of these forms as contemporaneous with the maximum extension of the Late Cretacic sea in the Andean geosyncline, and about of the same age as the littoral and sub-continental Puca sandstone of eastern Bolivia.

The genus *Echinocorys* appears, somewhat abruptly, in the European record during Turonian time, and although it may have, and probably did reach Europe from antecedent seas outside of Europe, the latter was obviously much nearer than was Bolivia to its center of evolution and dispersal. Hence the Bolivian occurrence cannot well be older than Turonian. The genus *Hippurites* is likewise characteristic of the Neo-Cretacic beds of the Mediterranean regions of the world and points in the same direction as does *Echinocorys*. It may therefore be concluded that the geological age is not older than Turonian, and is probably younger, namely either Emscherian or Campanian.

⁴ Voy. dans l'Amer. Mérid., tome viii, pl. 22, fig. 16, 1847.



NEW HIPPURITES FROM SOUTH AMERICA

CLIMATIC INFLUENCES IN VADOSE ORE
DEPOSITION

BY CHARLES KEYES

The desert, which according to Sir John Murray, occupies more than one-fifth of the land surface of the globe, and which, as we now know, profoundly affects another one-fifth, appears to be peculiarly the home of the metals. Iron alone is possibly an exception. Concerning changes with depth the ores of arid regions present many features which are altogether unknown in those lands with which most of us are most familiar. One aspect of the far-reaching influence which depth has on the character of ore deposits is of as great practical importance as it is of scientific interest; yet it is a feature which is quite generally overlooked. It is to this phase of the depth-factor that attention is here mainly directed.

Since we no longer can regard all ore-bodies as having been formed directly from igneous emanations arising from the depths we have to recognize the fact that there are really two great primary classes of ore deposits economically equally important. Those belonging to the class of deep-seated origin we shall have to pass over at this time with the mere mention of their existence. The other grand group, the one with which we shall have here particularly to deal, finds meager representation in humid countries where modern mining began. Its wide expansion in arid lands is such as to make it often the only class of ores over large tracts which is encountered or mined. Once thoroughly understood in regions of excessively dry climate the special features become easily recognizable also in other lands.

Certain of the broader aspects of the subject, as especially displayed in a humid climate, I have already considered in a paper

on the "Ozark Lead and Zinc Deposits; Their Genesis, Localization and Migration."¹ There is a somewhat more comprehensive survey of the underlying principles; there is also comparison to be instituted between moist-climate phenomena and those of an arid climate where the conditions are such as to admit of an enormous development of the vadose zone. The bearing of the latter upon the whole subject of metallogenesis is far-reaching.

Instead of being merely the weathered portions of old mineral-veins, formed from deep-seated emanations, as they are so often regarded, vadose ore bodies, as disclosed by arid region phenomena, appear not only to have an origin independently of that of ore deposits directly from the depths, but to have derived their materials from widely different sources. The amount coming from space is doubtless very much larger than it is customary to allow. Vadose ore deposits, furthermore, seem now to be susceptible of separation into subordinate groups having large taxonomic value and great usefulness in the development of mines.

It is to arid mining regions that we must turn for a quantitative evaluation of vadose ores among mineral deposits generally. Represented at the one extreme by impoverished, unimportant gossans of moist lands and at the other by the thick, usually rich, bonanza-based zones of excessively dry countries, the vadose region presents ore-forming conditions peculiar to itself. These ore deposits thus clearly belong to a class by themselves, as extensive, perhaps, as any originating directly from the depths.

The consideration of the vadose zone as apart from the profound region involves several important factors. The two fields are sharply separated. Their processes of ore-formation are wholly distinct. Rarely or only slightly do their limits actually overlap. The immediate sources of ore-materials in the one zone are quite remote from those of the other, and in the main entirely unrelated. In every respect the vadose ore-zone is independent of every other. Genetically, taxonomically, and economically, therefore, should it be treated by itself.

That the ore-phenomena of the vadose zone should never have received the special consideration which they deserve as a distinct class may be ascribed chiefly to the fact that they are usually superposed upon those of the depths. There is a mingling of

¹ Trans. American Inst. Mining Eng., vol. XL, pp. 184-231, 1909.

features. Had clear distinction between the two been possible early in the serious consideration of the subject of ore-genesis, the long controversy over the ascension and lateral-secretion hypotheses might never have taken place, and the true progress of our knowledge regarding ore-formation might have been far more rapid than it has been.

Until Franz Pošepný² so strongly emphasized, a few years ago, the fundamental differences between ore-forming conditions above and those below groundwater-level it was the well-known custom to regard the upper ores as merely the weathered parts of mineral veins. The distinction thus made was, as S. F. Emmons recently observed, one of the most valuable features of Professor Pošepný's now famous memoir. Of late years ores formed in the vadose zone have assumed an ever increasing genetic importance. No longer are they all regarded as simply the oxidized portions of deeper formed ore-bodies. Moreover, they do not necessarily have any immediate or direct association with profound veins. As an independent genetic group they appear in every way comparable to the known ore-bodies formed through deep-seated agencies. Commercially it may be questioned whether the strictly vadose ores do not constitute the major part of ores mined in the world today.

The genetic association of a universal bonanza zone with vadose ore-deposition is distinctly a result of arid-region experience. As recently emphasized, under conditions of excessively dry climate the vadose zone attains a depth and importance unknown elsewhere. From the relatively thin band which it presents in the normal moist land it not only expands in the desert to a zone of great magnitude but it becomes separable into recognizable subdivisions. These certain features which vadose ores present in arid regions have special genetic significance.

At the outset it is with great advantage that certain essential differences between the older mining conception of gossan and the newer idea of vadose zone are noted. In moist climates, where modern mining had its birth and greatest development, and where the weathered-rock zone is relatively thin, the rusty surface-ores naturally appear to be merely the oxidized portions of sulphidic veins and masses that have originated from the depths. As first

² Trans. American Inst. Mining Eng., Vol. XXII, p. 213, 1893.

demonstrated in arid lands, there is really among ore deposits an important facies which is not always conspicuously displayed under conditions of moist climate. At the bottom of the distinctly oxidized ores and between them and the so-called primary sulphide ores in fissure-veins, is a bonanza sulphide-zone, the materials of which are as truly products of vadose origin as are the rusty ores above. This is a distinction that Pošepný did not make; and the lack of it led him into insuperable difficulties.

Clear distinction between vadose ore-formation as a general geologic process and the superficial change of mineral veins to gossan as a local effect, appears to have been first pointed out in the instance of the lead and zinc deposits of the Ozark region. Coming at a time when Pošepný's paper was under warm public discussion Winslow's presentation³ of this phase of the subject did not attract the attention which it really deserved. With some objections removed, as I have elsewhere explained,⁴ and by the application of somewhat broader principles permitted by the investigations of another decade,⁵ a still wider generalization is not only possible but practically valuable.

Transferring the suggestions to the arid regions full data are at once obtained for the establishment of a great genetic class of ores, formed wholly and independently within the vadose zone. The validity of this group is well shown by the universality with which its various members are recognizable. Even in such a moist climate as that in which the Ozarks are situated we may have the apparent anomaly of an "eisener Hut" covering a sulphite ore deposit all the materials of which are derived directly and solely from vadose deposition.

In point of origin only a part of vadose ores really constitute gossans. Although it is not usually so regarded, the genesis of the ores of the vadose zone and of those originating from the depths of the earth is mainly and fundamentally different. On the whole, it is surprising at how few points the ores of the zones actually touch. That these two classes of ores were so long confused, or rather that they were not earlier differentiated, seems to be due chiefly to the fact that detailed investigations of ore-genesis were principally carried on in lands of moist climate where the

³ Missouri Geol. Surv., Vol. VII, p. 477, 1894.

⁴ American Geologist, Vol. XXV, p. 355, 1901.

⁵ Trans. American Inst. Mining Eng., Vol. XL, p. 184, 1909.

ore-bodies mined were distinctly of deep-seated origin. Desert mining regions, where the vadose zone is so very much more extensively developed than in moist countries, introduce to us many novel conditions.

The theory of gossan-formation assumes as its fundamental premise that the mineral-vein is of profound or magmatic origin and that the weathering of its surficial portion gives rise to the rusty oxidized ores. In general this is in fact only partially true, since the vadose region with which the majority of us are most familiar is the quite limited weathered zone of normally wet or moist countries. Under such conditions the subject of ore-deposition receives most attention; yet it is under these very circumstances that the distinctive features of the vadose zone are most completely intermingled and most thoroughly obscured.

In moist lands, as is well known, the chemical decay of rock-masses usually goes on at the surface of the ground much faster than the removal by erosion of the resultant rock-waste. In many mineral regions secular decay seldom extends downward more than a score of feet. Gossans formed under such conditions are more or less completely leached of their ore-materials. The relative small amount of ore not carried away from the upper weathered part of a vein collects at the bottom at groundwater-level as an insignificant "enriched" layer.

So inconsequential commercially is the enriched layer often that it is in many mining operations passed through unnoticed. Whenever important ore-bodies are encountered at the groundwater-level, their origin is commonly ascribed to causes other than the results of strictly vadose deposition as it is now known to be. The contrasts between ore-forming conditions in the vadose zone in moist and in dry climates are much stronger than might at first glance appear.

In contradistinction to the gossans of normal moist regions the vadose zone under conditions of excessively dry climate presents several peculiarities. In arid districts the absence of chemical rock-decay is especially noteworthy. Rock-weathering, or general rock-disintegration, takes place mainly through spalling or insolation. Great depth is one of the striking features of the vadose zone. Its great thickness gives exceptional control of ore-localization by geologic structure. The marked irregularity in depth of

the vadose zone finds no parallel in normally moist climates. Local impounding conditions of mine-waters give rise to the paradox of the occurrence of sulphide ore-bodies far above ground-water level in some localities, and of oxidized ores apparently far below it in others. A basal bonanza-band of sulphidic ores is developed to an extent unknown elsewhere. Recognition of distinct subzones is an unexpected aid to the interpretation of vadose phenomena.

In arid and in cold regions, as I have recently pointed out⁶ in considerable detail, rock-masses scarcely suffer any chemical change at the surface of the ground. Disintegration of the rocks is almost entirely mechanical in character; in comparison the chemical effects are quite insignificant. The slight amount of chemical decomposition which rock-materials undergo is well shown by the great talus-slopes and other accumulations of colluvial deposits which form veritable rubble-piles often of huge proportions and with materials so fresh to all appearance that they seem to have come directly from some gigantic rock-crusher. When examined under the microscope even the adobe soils of the arid region attest the strictly mechanical origin of the finer débris.

In the desert areas of western United States, for example, only in open mineral veins and in the crushed belts attending fault-planes does the scanty moisture accumulate and normal signs of chemical rock-decay appear. Not only do the rugged mountain ranges of the desert disclose little rock-decay but the substructure of the intermont lowlands rarely displays conspicuous evidences of chemical decomposition. As a factor in the general reduction of the land-surface towards sea-level, the chemical decomposition of rock-masses under conditions of arid climate may be entirely neglected.

Many of the broad intermont desert plains are covered to slight depths only by soil materials; nearly everywhere the beveled edges of the rock-strata are presented with little or no indications of chemical decay.⁷ Similar conditions are reported by W J McGee⁸ on the coastal plains of Sonora in Mexico. Passarge's wide observations in the South African deserts are of like import.⁹ Such

⁶ Bull. Geol. Soc. America, Vol. XXI, p. 569, 1910.

⁷ Bull. Geol. Soc. America, Vol. XIX, p. 63, 1908.

⁸ Bull. Geol. Soc. America, Vol. VIII, p. 991, 1897.

⁹ Zeitschrift der deutschen geologischen Gesellschaft, LVI. Band. Protokoll, p. 196, 1904.

conditions could hardly exist did rock-decay proceed to any marked extent.

Preconceived notions concerning the gossans of moist regions accord not at all with the facts observed in arid lands. Ore-bodies are formed that manifestly have no association whatever with thermal waters from the depths. "Enriched" ore-bodies it has been conclusively proven, often gather their metallic materials not from the remnants of former veins but from a wide area of the country rocks themselves, even when the latter are not ordinarily regarded as metalliferous. The ground-waters with their metallic salts in solution are directed along paths that are as immediately dependent upon definite geologic structures as are accumulations of rock-oil.¹⁰ This I have shown¹¹ to be also true in moist regions. Were Sandberger's principles of lateral secretion of ore deposits literally and alone applied to the vadose zone of arid regions, they would as truly meet actual conditions as does the ascension hypothesis the profound conditions.¹²

Under normal climatic conditions the continual breaking down of rock-masses at the surface of the ground is a process that is, as is well known, both chemical and mechanical in character. Although genetically entirely unrelated clear distinction between the two is not always made. In moist climates chemical decomposition of the rocks so greatly predominates over their mechanical disintegration that the effects of the latter process are in comparison scarcely noticeable. In the arid regions not only is the very reverse true but it is so to a preëminent degree.

The expansion and contraction which exposed rock-masses, rock-fragments and the components of the soils of the desert undergo on account of the great diurnal range of temperatures appears to be the principal method of rock-weathering. Insolation it is appropriately termed. The rock-surfaces are constantly kept fresh through the continual removal of the finer débris by the winds. In its larger phases Streeruwitz¹³ graphically describes the process as it operates in the hot, cloudless deserts of Trans-Pecos Texas.

Wind-scour, or deflation, is in fact far more potent than water-

¹⁰ Economic Geology, Vol. II, p. 780, 1907.

¹¹ Eng. and Mining Jour., Vol. LXXXVII, p. 1048, 1909.

¹² Trans. American Inst. Mining Eng., Vol. XLI, p. 140, 1910.

¹³ Texas Geol. Surv., 4th Ann. Rept., p. 144, 1892.

action in accomplishing the separation of the lighter minerals of the finer rock-waste from the heavier materials which tend to be left behind.¹⁴ I have lately¹⁵ quite fully discussed the various aspects of this subject as observed in the desiccated regions of southwestern United States and northern Mexico.

The remarkable distances downward to which typical vadose conditions prevail, despite the extreme irregularities of the zone, are especially emphasized in their recent consideration in the northern part of the Mexican tableland. In many localities of this region, as at Santa Eulalia, in Chihuahua, and in the Sierra Mojada, in Coahuila, 1000 and 1500 feet are not uncommon depths to be attained before groundwater-level is encountered. At Tonapah, Nevada, Spurr¹⁶ reports the Desert Queen mine dry at a depth of over 1100 feet. Recent writers, in calling attention to the great depth which the so-called gossan often attains in certain places in the dry belts of western United States, do not seem to appreciate the significance of the phenomena in its bearing upon ore-genesis: yet the facts recorded supply some of the most valuable available data. In this connection such descriptions as that by S. F. Emmons,¹⁷ of the Horn Silver mine, near Milford Junction, Utah, are especially instructive.

The great depths to which oxidized ores extend give rise to some unusual conditions. In some localities where geologic structures produce local impounding of mine-waters sulphidic ore-bodies are formed high above general groundwater level in the very midst of the oxidized ores. The great sulphide ore-lenses at Magdalena, New Mexico, which I have recently described¹⁸ in some detail, may have been produced in this way. The important point is that such sulphide bodies are not the remnants of originally deep-seated ores, but that they are formed within the vadose zone contemporaneously with the oxidized ores and from the same ore-materials, none of which need be immediately connected with primary sulphide ore-bodies.

The subject of depth as related to the vadose zone in desert lands requires especial elaboration which cannot well be given in the present connection.

¹⁴ Trans. American Inst. Mining Eng., Vol. XLI, p. 153, 1910.

¹⁵ Bull. Geol. Soc. America, Vol. XXI, p. 565, 1910.

¹⁶ Prof. Pap., U. S. Geol. Surv., No. 42, p. 106, 1905.

¹⁷ Trans. American Inst. Mining Eng., Vol. XXXI, p. 675, 1902.

¹⁸ Mining Magazine, Vol. XII, p. 109, 1905.

Although the fact of the great unevenness of the upper surface of the sulphidic ores in the dry regions is noted by many writers, it does not appear that any adequate explanation has ever been advanced. The controlling factor of this remarkable unevenness is to be sought in the peculiarities of arid climate. Under these conditions, as already shown, chemical decay of the rocks goes on only along fault-lines or joint-cracks; and groundwater-level lies deep. General rock-wasting proceeding mainly through mechanical means, the rock at the surface of the ground is altered but slightly, or not at all.

The great variation in depth to which oxidation proceeds is well illustrated in many mines throughout the arid districts. Some extreme cases may be noted. In the Caballos range, in southern New Mexico, glistening lead-sulphide bodies of considerable extent outcrop, whereas, only a short distance away, a mineral vein is completely oxidized to a depth of over 1000 feet. Similar noteworthy instances occur in the Spring Mountain mining district, west of Las Vegas, Nevada. Such marked irregularities often occasion no little uncertainty in mining operations.

Concerning this great and general irregularity of depth of the oxidized ores several important observations may be made. In consequence of the absence of general chemical decay of the rocks old sulphidic ore-bodies may be, through the effects of general erosion, or deflation, eventually uncovered and exposed at the surface of the ground in a perfectly fresh state, or with only a thin film or coating of the carbonates. The ancient mineral-vein, or ore-body, wastes away in the same way and at the same rate as the rest of the rock-mass.

On the other hand open fault-planes, joint-cracks or other fracture-lines in the rocks permit the meager supplies of meteoric waters to penetrate to great depths before finally reaching permanent water-level. Sufficient moisture accumulates along these paths to facilitate chemical alteration of both the ores and the wall-rocks. In dry regions migration of ore-materials doubtless takes place chiefly through means of the soluble chlorides¹⁹ instead of the sulphates, as under normal climatic conditions.

The occurrence of sulphidic ore-bodies high above groundwater-level in the very midst of the oxidized ores is often explained on

¹⁹ *Economic Geology*, Vol. II, p. 774, 1907.

the hypothesis that in arid regions the permanent water-level changes from time to time. In central Nevada, for instance, A. C. Lawson²⁰ ascribes such occurrences in the Robinson Mining District, near Ely, to a higher groundwater-level when the climate was much moister than at present. Others, notably J. E. Spurr,²¹ lay particular stress upon the wet climatic conditions during former geologic periods, as the Glacial Epoch, for example.

Plausible as is this explanation, it does not appear to be satisfactorily supported by unquestionable data. Changes controlled by climate are apt to be excessively slow. Until it is shown that these various occurrences of sulphidic ores within the oxidized zone are not the direct result of local impounding of waters due to favorable geologic structures, it may be seriously questioned whether the changes in groundwater-level have been so rapid or so marked as has been sometimes inferred. In a number of cases in which it was at first thought that decided oscillations of the groundwater-level had occurred it was found upon further investigation that the sulphide-bodies were disposed in basins in which, owing to peculiarities of geologic structure, impounding conditions of the groundwaters prevailed, producing, as it were, all the necessary factors characterizing the permanent groundwater zone.

That it is possible for groundwater-level slowly to change cannot be disputed. That the rate of such change is sufficiently rapid to be recorded in the oxidation of ore-bodies may be seriously questioned. That its oscillations are directly referable to great climatic swings between excessive precipitation and excessive drought is an assumption which may be safely denied. The seasonal variations in the volumes of water encountered in mines — as is notably the case of the Joplin zinc district, for example — have little critical bearing upon this point, since the extensive pumping of water from the mines produces artificial conditions. In moist climates the rainfall is so copious that the zone of permanent water saturation is always quite near the surface of the ground, and any oscillation of the groundwater-level would be hardly noticeable even under the most favorable circumstances. In arid lands where the vadose zone presents great extent the opportunity for recording oscillations of the permanent water-table is unexcelled; yet direct and unquestionable evidence in sup-

²⁰ Bull. Dept. Geol., Univ. California, Vol. IV, p. 331, 1906.

²¹ Bull. Geol. Soc. America, Vol. XII, p. 266, 1901.

port of the suggestion seems to be entirely lacking. When the subject shall have been especially investigated tangible proofs may be forthcoming.

In the arid regions of the Western Hemisphere, Latin-American miners recognize several distinct horizons or sub-divisions which are designated by appropriate names. These sub-zones have in fact some scientific foundation. They are distinguishable chemically, mineralogically and metallurgically. In mining operations their differences are taken into account. The recognition of these sub-zones seems destined to play an important part in the consideration of ore-genesis, as well as in practical mining explorations.

Many of the descriptions of mines in western United States indicate a certain sequence of ore-materials between the surface of the ground and the deep-lying water-table. The real significance of these observations is scarcely yet realized. The only writers inclined to lay especial stress upon this phenomenon are Fuchs and DeLaunay,²² who make out, in northern Mexico, no less than six distinct zones. These zones are based chiefly upon the mineralogic characters. The sub-zones are distinguishable upon other grounds. I have referred²³ to them in another place; and they are made the subject of special consideration elsewhere. In the present connection attention only need be called to their verity and importance.

Notwithstanding the fact that the process of ore-deposition and ore-reduction goes on mainly under the conditions characterizing the profound zone, the horizon of so-called secondary sulphide enrichment lying at and just below groundwater-level belongs properly to the vadose field. The ore-material there converted into the sulphide form comes chiefly, if not entirely, from above. Little if any is derived immediately from below.

Since the bonanza zone, or the zone of secondary sulphide enrichment, has become industrially so important, the question of the specific sources of its ore-supplies assumes new aspects of great interest to mining. Being generally regarded as mainly the result of downward percolating meteoric waters on low-grade veins, it gives strong support to certain facies of ore-genesis which

²² *Traité des Gites Minéraux et Métallifères*, T. II, p. 816, 1893.

²³ *Trans. American Inst. Mining Eng.*, (Bimon. Bull. No. 55), p. 547, 1911.

persons committed to a strictly igneous theory are loath to admit. This is a local phase; it has much broader aspects. The bonanza zone is properly to be considered not as a mere local phenomenon but as one of world-wide extent. The vadose zone of the ores may thus be viewed much in the same way as is the regolith, as having everywhere at its base a sub-zone of bonanza character, that in some places is sufficiently well-developed to be mined, but elsewhere often only feebly represented by ore-materials. The bonanza layer is the ultimate and maximum localization of vadose metalliferous materials of industrial importance.

In bulk the ore-materials of the vadose zone of arid regions must many times exceed the volume occurring in the accessible zone beneath the bonanza level. One of the main results of an extensive inquiry recently instituted in Mexico, for the purpose of determining the probable persistence of ore-bodies in depth, clearly indicates that in the large majority of cases little is to be expected. Only in exceptional instances is the outlook sufficiently favorable to warrant the continued exploration for possible deep-lying ores. Similar, but less comprehensive, observations conducted in certain parts of western United States strongly point to like conclusions. In the dry regions present estimates place the total volume of ore in the vadose zone at about ten times greater than that of the profound zone.

As bearing upon the immediate sources of the ore-materials in general these results are significant. They suggest that on the whole metalliferous materials are not segregated into ore-bodies so directly from the depths as it has been customary to suppose. They point to a larger importance of the metallic minerals diffused through the rocks and liberated by the latter's disintegration. They also indicate a larger influence of the meteoritic augmentations to the earth's supplies of metals as early advanced by Meyer,²⁴ and which later ²⁵ found strong support in the planetesimal hypothesis of the origin of our globe, as proposed by Chamberlin. These aspects of the subject I have also lately summed up.²⁶

It is in the arid regions that complete data on this theme are to be obtained.

From areas presenting large intrusive masses will doubtless

²⁴ Beiträge zur Mechanik des Himmels, p. 157, 1848.

²⁵ Carnegie Institute Yearbook, No. 3, p. 208, 1905.

²⁶ Trans. American Ins. Mining Eng., Vol. XLI, p. 139, 1910.

come the first quantitative data on the magmatic derivation of ore-materials. Laccoliths present about the only definite features which we know of concerning the expulsion of metallic vapors from cooling magmas. In those laccolithic masses which have come under special observation certain relationships are establishable between the metallic content of the cooled magma and the vein-phenomena displayed around the periphery. Ore-veins appear to form a surprisingly small proportion of the entire metallic content. In comparison, the part permanently retained as accessory components by the rock-mass seems to be large. The total volume of metal lost through expulsion as soluble compounds in the form of vapors is at best difficult to accurately estimate; but there is as yet no evidence to show that it is nearly so large as has been sometimes regarded.

In illustration, the granites and rhyolites of southeastern Missouri show no indications of the presence of metallic elements, yet Robertson's exact analyses²⁷ of perfectly fresh rock-samples demonstrate that large quantities of several metals are locked up in these masses. Calculated for a square mile of area and 500 feet in thickness the granites contain about 45,000 tons of galena, 65,000 tons of zinc-blende, and 135,000 tons of chalcopyrite; and the same volumes of the contiguous rhyolites, or porphyries, contain 75,000 tons of galena, 45,000 tons of zinc blende, and 60,000 tons of chalcopyrite.

At the southern end of the Rocky mountains, in central New Mexico, there is a remarkable group of four lofty laccolithic piles known collectively from the earliest times as the Gold Mountains. Their names are San Ysidro, Tuertos, Ortiz, and Los Cerrillos. On the Tuertos Group is situated the famous San Pedro Copper Mine. The areal exposure of the grano-dioritic porphyry of which it is composed is about five square miles. The country-rock itself assays about \$1.00 a ton,²⁸ and the mass thus contains gold equivalent to the present annual production of the entire United States. Although the first lode-mining in this country was carried on in the neighboring Ortiz district workable veins are exceptionally rare. At the San Pedro Copper Mines, the gold seems to be carried chiefly in the chalcopyrite.

²⁷ Missouri Geological Survey, Vol. VII, p. 479, 1894.

²⁸ Trans. American Inst. Mining Eng., Vol. XLI, p. 148, 1910.

TAXONOMIC SIGNIFICANCE OF PETER SANDSTONE

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In the course of field work during several seasons past on the Early and Mid Ordovician formations of the Ozark region much light appeared to be thrown upon the origin and stratigraphic relations of that great arenaceous terrane widely known throughout the Upper Mississippi Valley as the St. Peter, or Peter, sandstone. Starting out with the imposed assumption that this formation was eolian in its nature evidences soon became overwhelming that the beds really were ordinary marine sediments.

Inasmuch as a history of every geological formation is usually closely tied up with the histories of overlying and underlying terranes, it serves greatly to elucidate events to outline briefly the main features of Early Ordovician stratigraphy, and to place particular stress upon that of the Ozark uplift, making occasional comparisons with the salient characteristics of adjacent tracts.

The Ordovician section of Missouri, as delimited in the present connection, begins with the Roubidoux sandstone, which is believed to be the southern equivalent of the New Richmond sandstone of the north. Locally, at least, this formation embraces a thin basal conglomerate, composed chiefly of chert-pebbles presumably from the underlying Gasconade limestone. Other evidences of an erosion surface at this horizon are obscure. The Roubidoux formation varies from 75 to 200 feet in thickness. It consists of a succession of beds in which lateral gradations from sandstone into chert or limestone are the rule, and, while one section may be almost wholly sandstone, another may be over three-fourths limestone. Limestones become more abundant to the east, west, and probably to the south. In the size of grain,

the sandstones of the Roubidoux formation are almost identical with the Peter terranes. The grains are commonly well-rounded, although this feature is often greatly obscured by secondary quartz-enlargements. The formation is notably cross-bedded and ripple-marked, the ripples being of both the current and oscillatory types. Sun-cracks are also abundant, despite the fact that the formation carries marine fossils throughout. The fossils thus far found are limited to the cherty beds; none being found either in the sandstones or the limestones. According to the fossils submitted to E. O. Ulrich for identification the Roubidoux sandstone seems to correspond to the horizon of the lowest Beekmantown beds.

Above the Roubidoux sandstone, in slight local unconformity, rests the thin-bedded, argillaceous Jefferson dolomite, 200 feet or more in thickness, containing thin, discontinuous sandstone lenses. On the latter are ripple-marks, and sun-cracks occur sparingly; marine fossils are found at several horizons.

Following the Jefferson dolomite, with apparent conformity, is the cherty, argillaceous Cotter dolomite, with a dominant sandstone horizon near the middle. The maximum thickness, to the south in Arkansas, is 500 feet. A characteristic Beekmantown fauna is reported from these beds in Arkansas. The Jefferson and Cotter dolomites may be the equivalent of the Shakopee dolomite of the north.

The next formation, called Powell, which rests on the deeply eroded and weathered surface of the Cotter dolomite, consists of 200 feet of magnesian limestones, with some sandstones. It occurs as far north, in Missouri, as Ste. Genevieve County, and is the highest formation of Beekmantown affinities known in the state. The Powell beds of Arkansas are correlated with the uppermost Beekmantown section.

On the deeply eroded surfaces of the Beekmantown, or Canadian beds, and lapping northward directly upon the Late Cambrian strata in Wisconsin, rests the Peter succession. At its base in Missouri and Arkansas are a thick basal sandstone and a dolomite, together described as the Everton formation. The sandstone, which contains chert and limestone conglomerate and red residual clays at its base, is very variable in thickness, carries numerous thin, limestone lenses yielding marine fossils in Arkansas, is

well-bedded and ripple-marked, and is entirely indistinguishable on the basis of size of grain, purity, or degree of rounding, from the Peter sandstones above. The Everton limestone member, which is also quite variable in thickness, yields marine fossils in Arkansas. The total thickness of the Everton terrane in Missouri varies from less than 30 to over 75 feet. Traced northward, the Everton limestone thins out at the Missouri river, allowing the Everton and Peter sandstones to come together. Under these conditions they are wholly indistinguishable, and it is only on the basis of total thickness that both are believed to be represented north of this line. The Everton division is known to occur on the south flank of the Ozarks in Arkansas, and on their east flank in Missouri; but it is not known on the west flank.

Some slight emergence and erosion, probably greater to the south, than to the north, appeared to have taken place between Everton beds and the typical Peter sandstone. The Peter division, like the Everton, is not known in outcrop on the west flank of the Ozarks and its recognition in deep-well records is open to grave doubt. When the Everton beds are present, the Peter division varies from over 65 to 35 feet, and, north of any known Everton beds, averages 100 feet, a fact indicating the probable inclusion of the Everton sandstone (the limestone having failed) in the so-called Peter section. In southeast Missouri, and probably also in Arkansas, the Peter sandstone carries limestone lenses, and marine fossils were found at one point at the junction line between it and the overlying Joachim dolomites. Limestone lenses are also reported in north Missouri, in Iowa, and in Illinois, in deep-well records. Marine fossils are reported by Sardeson at three horizons, well down in the formation, near Minneapolis.

The unconformity at the base of the Peter sandstone in the northern states is undoubtedly the same as that at the base of the Everton beds in Missouri. The line marked in Missouri by notable relief, basal conglomerates, and residual soils, and similar phenomena are reported in Iowa and Wisconsin, the stratigraphic hiatus, and probably the time-value, increasing northward.

The degree of rounding of Peter sand-grains is comparable to that displayed in the Roubidoux sandstone, and in the sandstone lenses of the Jefferson and Cotter dolomites; as are also the size of grain, uniformity of grain, and degree of purity. The

absence of fossils is no more striking than that of the Rubidoux and Jefferson sandstone beds, but little more than that of associated limestones, fossils being limited in all these formations almost wholly to the beds replaced by chert. The Peter sandstone shows a less ratio of cross-bedding to bedding than the Roubidoux formation. Indeed, there is no valid reason for assuming any different source of origin for the sands of the Peter formation than for those of the older beds, which are undoubtedly marine.

The Joachim dolomite, which rests with apparent conformity on the Peter sandstone, seems to occupy the same stratigraphic position as the top of the Peter formation farther north. Both lie next below beds believed to be the Lowville-Black River equivalents of the East. Sand-grains of the Peter type, and even considerable lenses of sandstone, are common in the Joachim formation. Fossils are very rare in the formation, and are reported from but one or two localities. This is probably significant in explaining the rarity of fossils in the Peter sandstone. The surface of the formation underwent notable erosion before the Bryant [Plattin] beds were laid down over it, and the Jasper beds of Arkansas seem to occupy this interval. The Joachim dolomite is not known to occur north of Calhoun County, Illinois, and is believed by some to be the deep-water equivalent of the Peter sandstone.

Careful comparisons of the purity, degree of rounding, uniformity, and size of grain of the Peter sandstone and the sandstones in the section immediately beneath show conclusively that these beds do not differ in any appreciable particular, certainly not enough to demand any different explanation of origin. In fact, the suggestion is repeatedly forced upon the observer, that these sands were derived from a common source, and underwent practically the same history.

None of these sands, of which the Roubidoux and the Peter sandstones are the most important, could possibly have been derived from within the area in which they now lie. To the east, the fact that the Roubidoux equivalent, the Beekmantown, and the Peter equivalent, the Chazy section, are limestones with little sand, seems to eliminate Appalachia as a source. In the Ozarks, only a very small pre-Cambrian area, if any at all, was exposed as late as the time when the Beekmantown beds were first laid

down, and it is positively certain that little of the sand could have come from that source. Southward, the land-mass south of the Ouachita geosyncline was a source of important sediments, but these were largely shales, and it is inconceivable that the muds rested nearest the land, and the sands moved north across the geosyncline. Neither Roubidoux nor Peter sands seem to have come from this source.

To the west, in the Rocky Mountain region, nearly pure limestone was forming in Colorado during Beekmantown time, and it does not seem reasonable to look there for the Roubidoux sands. Since the Peter sands seem to have come from the same area that yielded the earlier sands, the north is the only plausible source.

The general history of the central Mississippi Valley region during Early Ordovician times is believed to be about as follows: Sands from a northern land-mass were delivered by rivers, and possibly directly by winds, to the Roubidoux-New Richmond seas, which extended north into Minnesota and Wisconsin; and were spread out by waves and currents. Limestone and shale-lenses were interbedded, and local emergences, with warping, allowed sun-cracking. As the land-mass to the north wore lower and lower, less and less sand was contributed, and the Jefferson and Cotter (Shakopee to the north) dolomites were laid down, with only occasional sand-lenses. A retreat of the sea, with the northern land-mass still low and furnishing little sediment, allowed the erosion of the surface of the Cotter formation, and a slight re-advance brought in the Powell materials over the southern half of the region without clastic sediments.

This movement was followed by a still more extensive retreat, producing the pre-Peterian unconformity. During this emergence, a relief of 200 feet or more was carved out upon the old Cambrian surface, with the removal of more material to the north than to the south.

The Peter sands which rest upon this erosion surface could not in any manner have been derived from the underlying rocks, and it has been shown that they probably came from the north, as did the Roubidoux sands. It is not believed possible that these sands were brought in as a great series of drifting dunes in an extensive interior desert. The rounding and frosting which are cited as evidence of this hypothesis are just as well-developed in

the Roubidoux sands, a clearly marine formation, and therefore afford no proof. The same is true of the size and degree of uniformity of the sand-grains.

The chert conglomerate at the base of the formation shows no evidence of wind-action. Bedding is more prominent than cross-bedding, and nothing like dune-structure is anywhere noted, even in the more protected valleys of the old erosion surface, where waves could not have worked it out. Marine fossils have been found in Arkansas in the basal Everton beds, the first deposit above the old erosion surface, as well as in the main body of the typical Peter sandstone in Minnesota.

The conditions of deposition of the formation, then, seem to be about as follows: After the post-Canadian [Beekmantown] emergence and erosion, the sea advanced from the south, spreading over the southern edge of the continental platform from the Ouachita geosyncline, from which it may never have completely retreated. The first advance found a low limestone land-mass with a mantle of ordinary residual-clay soil. This is partly worked over; but owing to the character of the relief, which was obviously not completely planed down by wave attack since it still exists, some of this residual clay was preserved in places.

Across this area from the north flowing rivers delivered some sands from northern sources, but the earlier submergence at the south was accompanied by the formation of considerable limestone, representing the lower Simpson beds of Oklahoma, and, with the still further encroachment of the sea, the Everton beds of Arkansas and southern Missouri; the sand being locally and irregularly distributed. During this stage it is quite probable that along the valleys of the main streams emerging from the supply ground to the north there may have been local dune areas, just as there are now along the valleys of rivers generally that carry much sand.

After the shore had progressed northward at least past the central region of Missouri, and possibly far up into Iowa and Illinois, closing the period of Everton deposition, there appears to have come a warping which caused the emergence of the Ozark region, allowing the erosion of the surface of the Everton limestones.

It is entirely possible that this emergence may have brought

out of the sea large areas of already deposited marine sands to the north of the limestone facies, allowing them to drift and become further rounded and sorted. The chief rounding was not produced in this way, since the formation consists of well-rounded grains quite to its base. This emergence appears to have been followed by a tilting which submerged the area to the south and uplifted the source of supply somewhat, and it was following this episode that the great bulk of the Peter sandstone was laid down.

In a formation of the character of the Peter sandstone, it is difficult, if not quite impossible, to say with certainty just where the shore was at any given time. That the Peterian sands often encroached from their supply ground upon the surface of the old Canadian or Beekmantown tracts before the sea covered the entire area, is certainly to be expected; and this was probably accomplished both by streams and by dune-drifting. Such local dune areas may never have been completely worked over by the waves. Twenhofel describes cross-bedding which he thinks is probably of eolian origin. Cross-bedding is perhaps more highly developed in Wisconsin than in Missouri. This would be true, even though the formation was entirely marine, because in the near-shore zone of more active wave-work, cross-bedding would be the more to be expected. Nevertheless it would not be surprising, even though the Peter sands were dominantly marine, to find some dune-structures in this northern area. It is already pointed out that along the trunk streams, dune-drifting may have gone some distance on either side. Elsewhere than along main drainage lines it was probably of only very local extent. Unless we assume a profound modification of the earth's axis of rotation, the area under discussion must have been then, as now, under the prevailing westerly winds. Within this area, at the present time, there is little evidence that sands would drift southward in large quantity. In fact, in discussing the origin of the Sylvania of Michigan, it has been postulated that it was derived from the Peter, and older, sandstones of Wisconsin, driven by the wind from west to east. If the prevailing direction of the wind in Monroan time was from the west, surely no logical reasons can be advanced to show why it was any different when the Peter sands were deposited.

The objection has been raised that a sea encroaching over

this central area of marked relief would surely have planed away the hills of the old land-surface, had they not been mantled by sand. To this it can be replied that in Arkansas, where there is no visible notable relief in the top of the Canadian or Beekmantown formations, the first deposit laid down was a marine, fossiliferous limestone. In Missouri, on a surface of large local relief (over 40 feet in 100 yards) the first deposit over a considerable area was a complex of interbedded limestones and sandstones. In these cases the waves clearly were not prevented from planing away the hills by any protecting mantle of sand, and still the hills remain. The objection, therefore, has little weight.

At the time of the Peter deposition it is believed that the prevailing continental slope was southward. The irregularities of depth carried by the hills and valleys would surely exert a profound influence on currents, and since it is quite impossible to map the shoals and possible islands in the Peter sea, it is quite as impossible to describe its currents. These were undoubtedly of importance in distributing the Peter sands.

An objection is sometimes advanced that the hills and valleys would prevent distribution of sands by the waves. On the other hand the same features are cited as barriers to dune-migration. Neither interpretation seems to be well founded, since in either case the sands would, as they migrated, simply fill each valley until it began to spill over the ridge, into the next valley. In this way, there could gradually be established on the submarine slope a gradient which would ultimately become a graded slope for the given depth, size of waves, strength of currents, and supply of sand. Gradually, from time to time, by warping, shifting of shore-lines, or modifications of the rate of supply of sand, this gradient would be rendered flatter or steeper.

It does not seem to be necessary, as so many have supposed in the past, that there must be an advancing or retreating shore-line to enable the sea to spread out an even blanket of sand. With a constant source of supply, and a stable land-mass, there seems to be without doubt that such a graded slope would be established, down which the sand must move so long as the basin was not filled up. As the basin becomes shallower and the gradient less, the rate of advance of the sand-mantle would become exceedingly slow, just as in the final stages of peneplanation.

Probably the Peter sands were actually laid down in deeper

water than the Roubidoux sands, since no sun-cracks have ever been noted in the shaly layers in Missouri. Such cracks are very common on Roubidoux surfaces, in similar layers, and at many horizons. Their absence in the Peter formation, then, indicates not only that the materials were laid down under water, but that it probably was not subject to as frequent emergence as in the case of the Roubidoux beds. Farther north, and in closer proximity to the shore-line, it is more than likely that there were occasional emergences which brought local shoals within the range of wind-action for considerable periods of time, and this may well account for the fact that some samples of the Peter sandstone show a much greater degree of rounding than others, even when the effects of secondary enlargement are eliminated from consideration.

The character of the land-mass from which the Peter sands were derived is a point of moment. This, as already shown, was in general to the north of the present belt of the Peter formation, and largely occupied the area commonly known as the pre-Cambrian, or Canadian Shield, extending for unknown distances to the north, the northeast, and the northwest.

The region was probably not arid. The rainfall of the Upper Mississippi Valley at the present time, is controlled very little by the matter of elevation, and over large areas practically not at all. The important control is the cyclonic storms of the prevailing westerly winds, drawing their chief moisture from the Gulf of Mexico. Since the area under consideration exhibited when the Peter sands were being deposited, a relation of land and water very similar to that now existing, there is certainly no reason to doubt adequate precipitation. The fact that the rock materials are very completely weathered also suggests at least moderate precipitation. Even the Potsdam sandstone derived from this land-mass carries very little undecomposed feldspar, except at its very base.

Throughout Potsdam deposition, the land-mass probably never stood very high, and this is evidenced by the fact that weathering seems always to have kept well ahead of erosion, so that no coarse conglomerate material was developed, except in the immediate vicinity of old monadnocks like the Baraboo Range. The lack of feldspathic constituents in the resulting sediments also indicates

that weathering proceeded well in advance of the removal of the residual soil.

The land-mass was apparently without vegetation, for, if it may be judged by the best available evidence the world over, land plants had not yet developed in Early Ordovician time. If they had, they were probably of minute size and easily destroyed. Practically no important remains of land plants are found before Late Devonian times. This absence of vegetation probably was an important factor in increasing the efficiency of wind-work, since it must have given even to a humid region many of the characteristics of a desert.

The wind circulation must, unless we assume profound shifting of the pole, have been much as at present. This factor, together with the other points already cited, the absence of vegetation, the comparatively flat topography and the deep weathering, seem to constitute vital points in the proper understanding of one of the apparent anomalies of the early Paleozoic sediments of the north central United States, namely the remarkable rarity of shales as compared with sandstones, from the base of the Cambrian section to the top of the Peter sandstone. The average igneous rocks should yield several times as much clay as sand. The early Paleozoic sediments, derived from the pre-Cambrian shield, afford several times as much sandstone as shale.

In such an area as postulated above, with its continental slope to the south, with deeply weathered soils, and with no protective cover of vegetation, westerly winds sweeping across the drainage lines would be given the maximum power of removing the clay in actual suspension, while drifting the sands into the rivers to be carried south into the sea. The dust, which was probably not carried far at any one time, except during unusual storms, came to rest to the eastward, each time on land instead of being dropped into the sea, until finally blown far eastward into the Atlantic Ocean and lost beyond possibility of study. This process would account for the fact that the Potsdam formation, though it has many shaly beds, contains far less than the theoretically normal amount of clay. The wind drift would also aid in producing the perfection of rounding seen in these sand-grains.

With the close of Cambrian time and the northwardly encroaching Ordovician sea, the southern fringes of the Potsdam sandstones

were probably buried beneath overlapping limestones, and the land-mass, now well worn down, could contribute but little sand to the Canadian seas of the Mississippi Valley region, and that little even more completely sorted than before. The emergence in post-Cambrian time first exposed a surface capable of yielding but little sand, but as the northern fringe of the Canadian or Beekmantown beds was gradually removed, and supplied calcareous material to form the Everton formations, a broad belt of Potsdam sandstone was likely ultimately to be uncovered. This seems actually to have occurred, since, as already pointed out pre-Peterian erosion in one area in central Wisconsin even now shows the Peter sandstone resting on the Cambrian Jordan sandstone.

With such a belt of Potsdam sandstone exposed along the border of the crystalline mass as now fringes the pre-Cambrian shield in central Wisconsin, but farther to the north of course, conditions would be particularly favorable for maximum wind action. While some sand would continue to be contributed from the crystallines to the north, much would now be derived from the newly-exposed Cambrian beds. This material, already once sorted over under very favorable conditions for rounding and removal of clay, would now undergo a second rounding and sorting of the same character, and when laid down as Peter sands might be expected to show a remarkable degree of purity.

It is believed that the sea, in which the Peter beds were laid down, entirely submerged the Ozark region, and that the Peter sandstone once entirely covered that area. There are two chief lines of argument to substantiate this view. The first is that Peter formation maintains its high degree of purity everywhere around the present Ozark border. Had there been large areas of Canadian beds exposed to erosion on an Ozark island, the bordering Peter sands would surely have carried much clay and considerable cherty gravel, of the sort that the Ozark streams are now transporting in such abundance from the surface of the former. The second line of evidence is the wide distribution over the Ozark upland of patches of sandstone very similar in grain to the Peter formation. In fact, much of it was called Peter sandstone until the discovery of pockets of coal associated with it, and locally of Mississippian fossiliferous chert boulders beneath it, since which date it has been classed as Coal Measures.

Now the Carbonic sandstones in adjacent regions do not carry these well-rounded grains, and it is therefore believed that much of the Peter sandstone formerly present over the Ozark crest, may have been destroyed to produce the Coal Measures sandstones of this region.

As the shoreline gradually shifted northward, limestone deposition became dominant in the south, and the Joachim dolomite of Missouri and Arkansas represents, perhaps, the same horizon as the top of the Peter sandstone in northern regions.

After deposition of the Peter sandstone, there seems to have been emergence in the Ozark region, as witnessed by the fact that the next formation rests on the eroded surface of the Joachim dolomite. Slight evidences of such erosion are seen at the top of the Peter sandstone farther north, but there is some reason to think that the emergence was greatest in the Ozark region. This emergence does not appear to have rejuvenated the northern land-mass, now worn so low that it was furnishing practically no sediment at all.

With the re-advance of the sea, which seems to have come in very widely over the region, without a measurable time occupied by its spread, there suddenly appeared an abundant fauna of the general Lowville-Black River type, marking the base of the Bryant [Plattin] dolomite in Missouri and Arkansas and the Platteville dolomite of the Upper Mississippi Valley. If the Everton, Peter, and Joachim formations, and perhaps Jasper beds of Arkansas, be considered the Minnesotan series, then its deposition began earlier to the south, with considerable limestone, apparently before the main source of the sands, the Potsdam sandstones, had been uncovered at the north. With the influx of the added sand supply when erosion tapped this source, the sands encroached southward on the limestone areas, but as the land to the north was worn still lower, so that the supply became more limited, the limestone area again encroached northward over the sands, as at the Joachim horizon. The base of the Minnesotan section to the south (the Everton) is older, then, than any part of it in the north; while the Joachim dolomite in the south seems to be nearly coincident with the top of the Peter sandstone in the north, since both seem to rest beneath essentially on the same horizon.

It seems quite probable that the low, flat condition of the northern land-mass at the close of the Peter deposition allowed the Lowville-Black River sea to lap far up on the old crystalline shield. When Decorah deposition set in there appears to have been some disturbance or slight uplift to the northwest, but not in an area that exposed any of the older sandstones to erosion. In any event, the source was such as did not yield sand-grains; and the Decorah terrane thickens appreciably to the northwest, to the point where it was cut off by Jurassic or Comanchan erosion. The top of the Decorah shale, where thickest in the vicinity of Minneapolis, may be the equivalent of the basal Galena dolomite farther south, the shale grading into limestone as the distance from the land source increased.

Following Decorah deposition there seems to have been an even wider submergence during which the sea probably covered much of the Canadian shield from Wisconsin north to Hudson Bay and from Lake Temiscaming west beyond Winnipeg. This submergence, also, it is believed, entirely overspread the Ozark region, or if not, left only a very low island of limestone, not deeply eroded. The absence of any sandy layers such as would surely have resulted from erosion of the Peter sandstone makes it certain that that formation was not then exposed.

Finally the area of the Mississippi Valley again emerged, and remained land during most of the time when the Eden and Maysville beds were being deposited. Wide submergence resulted in the laying down of a thin limestone, the Fernvale formation, over much if not all of the Ozark region and far to the southwest; while in the north, beginning simultaneously, or possibly a very little later, came the great influx of calcareous muds that produced the Maquoketan shales. This formation is more limey to the north, as represented by the Wykoff beds, and more sandy to the south, as indicated by the Thebes sandstone, and suggests a change in the direction of the source of sediments. Mid Ordovician submergence seems to have closed a portion of geologic history when the Canadian shield was an active source of important Paleozoic sediments.

SECULAR CHANGES OF GEOLOGICAL CLIMATES

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In his masterly essay on "Climate and Cosmology" Croll astutely observes, that the most important problem in terrestrial physics . . . and the one which will ultimately prove the most far-reaching in its consequences, is: What are the physical causes which led to the Glacial Epoch and to all those great secular changes of climate which are known to have taken place during the geological ages?

In attempting to interpret the secular changes of past climates upon a basis of limited and fluctuating supply of earth-heat and its cognate energy, radio-activity, converted into heat, under the highly conservative conditions imposed by water in its various forms, and the utilization of solar energy as a conservative factor throughout geologic time, as competent to maintain the narrow limits of temperatures known to have prevailed; and under these conditions a limited supply of earth-heat is recorded as a controlling factor during all geologic periods until the inauguration of the sole control in the Modern Era of the greater and more constant source, solar energy, appeal is made only to those factors which have probably always been of importance in determining the character of climates.

That a logical application of the known principles of atmospheric physics, of the heat-conserving functions of water in its several forms and their action upon radiant energy, of the intermittent liberation of heat from the non-conducting crust by ruptures and by its exposure to denudation and to the setting free of radio-active energies, of the further conservation of this heat through the utilization of solar energy in and beyond "the true

radiating surface" of the planet are self sufficient satisfactorily to explain all variations and special developments of past geologic and present climates; and that all climatic phenomena come within reasonable explanation after the rejection of the assumption of solar climatic control previous to that era during which time control is distinctly demarkated in zones of climate, are some of the conclusions to be drawn. Detailed discussion of these several factors are recently set forth in my essay on the "Evolution of Climates."¹

These conclusions seem to be so fundamentally different from those reached by others who have essayed these problems that they are here specifically recapitulated.

That solar control alone prevailed during any period of geological time appears untenable. The basis of this rejection is the complete failure of any and all attempts to fit the facts of geology thereto, and the contradictions and anomalies which such control cannot meet. This carries with it the rejection of those mathematical calculations limiting the time and effects of earth-heat influences, which are used to fortify the assumption of solar control. The rejection of these results carries with it the inadequateness of the assumed factors and the omission of the larger and more important ones, which controlled the conservation of earth-heat. These calculations were made before the sources of heat rendered available by the exposure and transformation of radio-active materials were discovered.

That this heat, conserved by a non-conducting crust, and stored in the oceans and liberated as a climatic factor by slow denudations and exposures of radio-active materials, by faults, fractures, ruptures, volcanism, and other changes of its topographic forms, was a factor in temperature conditions until the exhaustion of its last effective increments from the oceans in Pleistocene time seems clear.

That the supply of ocean-stored heat, replenished from time to time by the above processes, kept the seas warm until near the close of geologic climates is attested by the character and distribution of fossil marine life, by the distribution of temperatures and of ice. They fluctuated through very moderate limits and fell to glacial temperatures only in Pleistocene time.

¹ Evolution of Climates, Pamphlet, 66 pp., Baltimore, 1922.

The oceans, at these temperatures, do not generate sufficient water-vapor to maintain more than the present 52 per cent of cloudiness which admits solar energy to the lower and denser strata of the atmosphere and to the earth's surface, which it warms. The earth then emits long wave radiations which are trapped or absorbed in the atmosphere. This process, within certain reasonable limits is cumulative, and results in a gradual amelioration of the conditions of Pleistocene time. The oceans at the higher temperatures of pre-Pleistocene time maintained a far denser and more continuous cloudiness, which shut in earth-heat, and intercepted and utilized solar energy in the upper atmosphere as a further conservative influence.

- That the interposition of this cloud-sphere imposed two highly conservative conditions upon the further conservation of earth-heat: (a) The moist air and clouds are practically impenetrable to the radiations emitted by the planetary surface, and the loss of this heat through these media was restricted to the interchange of water in its various forms in the atmosphere and of the action of cyclonic and anti-cyclonic circulation as fixed by solar radiation; (b) solar radiation as a source of heat was restricted to the part of a conservator of earth-heat by its absorption and utilization upon and above the cloud-sphere, and was admitted to the lower regions of the atmosphere and to the planetary surface as a controlling factor by reason of impairments in the cloud-sphere.

That continents thus protected from solar radiation, and by reason of low specific heat and elevation, frequently reached Glacial temperatures during the intervals between outbursts of earth-heat from the forming crust, during which intervals adequate earth-heat was not available to prevent glaciation, nor was solar radiation available by reason of the intercepting cloud-sphere. They were also exposed under the same condition to two periods of maximum glaciation, first in the regions of cold, down-cast currents on anti-cyclonic latitudes, and later to the chill of final refrigeration in Pleistocene time.

That during geologic time, as now, the functions of solar radiation prevailed in fixing zones, or belts of cyclonic and anti-cyclonic circulation, of maximum and minimum barometric pressure of consequent cloud densities and belts of maximum and

minimum precipitation, but its heating or temperature effects were intercepted by clouds maintained by warm oceans and the available supply of earth-heat inside the constant temperature chamber of moist air and clouds. Under these conditions the *supply* of water-vapor was dependent upon the stored heat of the oceans, and its *disposition* as a circulating agent in the form of vapor, clouds, rain and snow was fixed by solar radiation acting on the spheroid of air and clouds, these effects in no material way differing from the present dispositions, except more uniformity in the exposed surface than at present offered by land and sea and variability in clouded areas.

That upon the acquisition of a stable crust by the slow processes of cooling, and, by the overloading of continents at various periods with glacial ice, and the removal of these overloads under the effects of solar energy, the liberation of increments of heat ceased, and the oceans chilled to their point of maximum density. This degree is too low to generate sufficient water-vapor to maintain continuous cloud density at any latitude, and leaves surface temperatures under the control of solar radiation. Under this control glaciation is not possible until this source of energy shall decline to an effectiveness less than that now reaching polar latitudes. That should this period arrive a type of glaciation not yet recorded would follow.

Under the rewarming effects of solar energy temperatures must continue to rise until the surface of oceans shall rewarm to a degree which will increase evaporation therefrom and impose a check to further rise in temperatures by increased cloud extension and density.

Within the field of speculative cosmology future glaciations of the earth are possible from two causes: (1) In the process of stellar evolution, at some remote eon of time, solar energy may decline to a maximum efficiency less than that now deglaciating polar latitudes and less than that now seasonally melting off the polar snow-caps of Mars. The ensuing glaciation would be unlike any yet recorded; it would be inaugurated in polar latitudes and it would be light, since cold and freezing oceans do not yield sufficient water-vapor for severe glaciations. (2) A cataclysmic rupture of the crust which would liberate sufficient earth-heat, now locked within its non-conducting crust, to rewarm the oceans.

This would re-inaugurate geologic climates and the ultimate chilling of land-masses, with still warm oceans, and resultant dense clouds would give a corresponding series of climatic events to that recorded within the reach of the geologist.

It is not improbable that such phenomena may have characterized pre-geologic eras; and that the eras of geologic history are only the last chapters of the formation of the fully tested and stable crust.

Under neither of the above possibilities may future glaciation be regarded as an impending event.

The conclusions applied apparently have analogies offered by other planets. Such application can now be made only under a broad interpretation of the features presented by the planets which are selected for this purpose.

If the principles herein used for the interpretation of the climates of the earth have been rightly selected and applied, and the conclusion correctly reached and found to fit the geologic record and present conditions, these conclusions have certain general applications to the other members of our solar system. This is well brought out by Herschel, Chamberlin and Salisbury, Barnard and Campbell.

During the greater part of geologic time, it is held that the earth was swathed in clouds ranged in zones parallel with the equator. This cloud-sphere was practically continuous, and presented a surface of high albedo — that of clouds, or about .60. At present this surface is broken, and the albedo has been lowered by the exposure of about 48 per cent of its dark surface. Moreover, it is known that the violet end of the spectrum is utilized to a greater extent than the red end, and hence this end controls the color of the reflected ray.²

It follows that a planet in this, or in a later, stage of climatic development has a low albedo, and the color of its reflected light is that of the least utilized end of the spectrum. The earliest of these stages must be presented by the larger planets, whose masses have imposed a longer period of cooling, and they present a cloud-banded surface of high albedo.

The latest stage must have been reached by the smaller planets

² Hence the copper red of the "earth shine" on the new moon, which reflects greatly reduced but normal light from the crescent.

which, having cooled more rapidly, have reached a more advanced stage, and present a surface of low albedo, utilize the violet end of the spectrum, and reflect deficient in this color.

The first stage appears to be presented by the great planet Jupiter, which seems to be shrouded in dense clouds maintained by its own heat, and exposes a surface of high albedo 0.62, with banded zones and spots having varying tangential velocities and moving freely in its atmosphere. The heat emanating therefrom is about of that intensity which should be derived from reflected solar energy. The reflected light is normal and white, which shows that neither end of the spectrum is utilized or trapped to a greater extent than the other, as is the case with Earth and Mars. All the conditions are similar to those which a densely clouded earth would present to an observer in interplanetary space.

Mars, on the other hand, presents a clear, or slightly cloudy, atmosphere, through which the features of the surface are faintly observed, notably the alternate formation and melting away of polar-snow caps as these are seasonally turned away from or toward the sun. The albedo is low, only .26, the violet end of the spectrum is manifestly utilized to a greater extent than the red end, and hence this latter color prevails in the reflecting light.

Polar ice-caps are lacking, and in their stead polar snow-fields form in winter and melt off in summer, foreshadowing the conditions of the earth's polar regions when energies and conditions now active shall be deglaciaded, first the north polar regions in the land hemisphere and later the ocean-bound regions of Antarctica.

Three distinct stages of climatic evolution are therefore apparently presented by these three planets. Jupiter, in the stage through which the earth has passed; the Earth in the stage of the gradual development of solar controlled climates; Mars in a more advanced stage towards which our present developments are tending.

If the eras of climates through which the earth has manifestly passed and the changes now passing before us have herein been referred to their proper principles, and correctly interpreted, the "intricate problems which have hitherto baffled the geologist" may prove grander by reason of their simplicity and unity.

ERAL AFFILIATIONS OF GRASSY BLACK SHALE

BY CHARLES KEYES

As a terranal title the term Grassy Shale was originally applied in its present sense in 1898, to a thin black section beneath the Louisana limestone in northeastern Missouri. Recent collection of a considerable assemblage of fossils from these shales, which were long regarded as absolutely barren, presents a number of novel and critical features bearing directly upon the taxonomic affinities of the formation.

By curious stratigraphical coincidence the black zone is a median member of a four-fold shale section forming an unbroken shale sequence on the Devono-Carbonic boundary. To this singular circumstance is ascribed much of the early misinterpretation regarding the geologic age of these beds.

The Black Shales, of which the Grassy black shales of Missouri may be, partly at least, a northern extension, were early observed in the continental interior. In Indiana, Owen¹ called attention to the Black slate lying between the Encrinital limestone (Early Carbonic), and the limestones of the Falls of the Ohio River (Devonic). Coming along two or three years afterward Hall² proposed to recognize in these black shales of the West the Marcellus beds of the New York section. After Owen and Norwood³ described the "Protozoic and Carboniferous Rocks of Central Kentucky" and regarded the Black shale as the basal member of the Carbonic section of that region, Hall⁴ revived his earlier opinion and correlated the western formation with a black shale younger than the Marcellus formation (Mid Devonic), referring

¹ Geol. Surv. Indiana, Second Ann. Rept., p. 17, 1839.

² Trans. American Assoc. Geol., 1842-3, p. 280, 1843.

³ Researches among Protozoic and Carboniferous Rocks of Central Kentucky, Pamphlet, 40 pp., 1847.

⁴ Am. Jour. Sci., (2), Vol. V, p. 269, 1847.

it to the Genessee shale (still Mid Devonian). De Verneuil,⁵ after viewing the contained fossils in the field, sided with Owen as to its Carbonian age. In 1866, Meek and Worthen⁶ were still calling this Black shale by the New York name, the Genessee slate.

On the Mississippi River the Black shale was noted as early as 1855 by Swallow,⁷ although only incidentally. A few years later it was briefly described by Meek and Worthen,⁸ who had examined the formation on the east side of the great stream. In both of these allusions the entire section of shale immediately beneath the Louisiana limestone was considered. This was also the case with the other references to this shale in Indiana and Kentucky. Whether or not this section was an exact terranalog equivalent of the Chattanooga black shale of distant Georgia, as in recent years described by Hayes,⁹ remains to be shown by further field correlations. Ulrich¹⁰ was inclined to extend the title Chattanooga Terrane of the southern Appalachians northwestward to Iowa, including by that term both the Green (Saverton) and the Black (Grassy) beds, exposed at Louisiana, Missouri, in the one formation.

When the Black shale of northeastern Missouri again comes into prominent notice¹¹ the shale section is divided into an upper Green member, and a lower Black member. Both of these beds develop westward into heavy bodies having a combined thickness of more than 100 feet.¹² So important are the sections a few miles from Louisiana, on Grassy, Noix, and Buffalo creeks, that their terranalog significance requires distinct names. The Black shale is designated in the fieldbooks of the Survey, the Grassy Creek shale. Soon afterwards this distinctive geographic title is formally published as a terranalog term.¹³

Since the stratigraphic importance of the black Grassy shales comes to be fully recognized it is carefully traced northward¹⁴ and southward¹⁵ from Louisiana. Although at the latter point it is only 4 feet thick the formation attains a much greater vertical

⁵ Bull. Geol. Soc. de France, T. IV, p. 13, 1847.

⁶ Illinois Geol. Surv., Vol. II, p. viii, 1866.

⁷ Missouri Geol. Surv., 1st and 2nd Ann. Repts., p. 107, 1855.

⁸ Am. Jour. Sci., (2), Vol. XXXII, p. 167, 1861.

⁹ Bull. Geol. Soc. America, Vol. II, p. 143, 1890.

¹⁰ Bull. Geol. Soc. America, Vol. XXII, p. 608, 1911.

¹¹ Bull. Geol. Soc. America, Vol. III, p. 286, 1892.

¹² Missouri Geol. Surv., Vol. IV, p. 47, 1894.

¹³ Proc. Iowa Acad. Sci., Vol. V, p. 63, 1898.

¹⁴ Am. Jour. Sci., (4), Vol. XXXVI, p. 160, 1913.

¹⁵ Proc. Iowa Acad. Sci., Vol. XXIII, p. 113, 1916.

measurement westward. Before disappearing below river-level in the Keokuk syncline, it reaches a thickness of 30 feet. In sections at Keokuk it is not definitely recognized or separated from the associated shales. At Morning Sun, north of Burlington, it is distinctly present in a number of deep-well sections. It is traced farther north to beyond Muscatine, where Udden¹⁶ gives it the name of Sweetland formation. There it is 45 feet thick, rests in notable unconformity upon the Devonic limestones, and has resting upon it unconformably the Des Moines coal measures.

The Grassy shale is of exceptional paleontological interest at this time. Despite its associated faunal affinities it doubtless represents the nethermost member of the Carbonic section in the Upper Mississippi Valley. At Louisiana this shale reclines directly upon Siluric limestone. A few miles away it lies immediately upon the Callaway (Devonic) limestone. Farther on, what appears to be the Lime Creek shales are found next beneath. At its base, therefore, a marked unconformity exists, which is also well displayed at the north, above Muscatine.

As recently noted¹⁷ the settlement of the Grassy black shale with the Carbonic section seems to set at rest several moot questions. It doubtless represents a part of the Chattanooga black shales which, in the South, appear, according to the best authorities, to constitute the base of the Mississippian series. It is not to be regarded as Devonian in age as suggested by Udden.¹⁸ It is not a local development of uncertain affinities, as stated by Calvin;¹⁹ nor does it underlie the Lime Creek shales as indicated in his general section²⁰ of Iowa. Thus it appears that Owen and Norwood,²¹ in drawing the line of separation between the Devonian and Carbonic strata in the Mississippi valley at the Black shale, displayed phenomenally keen insight into the real geologic succession of the region.

As also recently observed the Burlington Kinderhook section remains for further explanation. The disposition of the terranes admit of another interpretation for that presented.²² When the Devonian Interval in Missouri²³ was under discussion the inclina-

¹⁶ Iowa Geol. Surv., Vol. IX, p. 289, 1899.

¹⁷ Am. Jour. Sci., (4), Vol. XXXVI, p. 162, 1913.

¹⁸ Iowa Geol. Surv., Vol. IX, p. 301, 1899.

¹⁹ Journal of Geology, Vol. XIV, p. 572, 1906.

²⁰ Iowa Geol. Surv., Vol. XVII, p. 192, 1907.

²¹ Researches on Protozoic and Carboniferous Rocks of Central Kentucky, 1847.

²² Am. Jour. Sci., (4), Vol. XXXVI, p. 163, 1913.

²³ Bull. Geol. Soc. America, Vol. XIII, p. 69, 1902.

tion was to regard the entire shale section between the Callaway limestone and the Chouteau limestone as a distinct and compact lithologic unit, Devonian in age, perhaps, but having intercalated the lens of Louisiana limestone. This conclusion was based partly, perhaps largely upon faunal grounds, and especially upon the character of the *Gomphoceras* fauna then newly found well up in the section at Burlington, in what is doubtless a horizon of the Saverton shale, there merging without evidence of break with the Hannibal shale. Afterwards the fauna was noted by Weller.²⁴ It was discovered by me at the time that the report on Des Moines County was being printed;²⁵ and six years later the fossils were turned over by Doctor Calvin to Professor Weller for critical examination. As one of the results Weller was led to correlate²⁶ the lithographic limestone (bed 4) of the Chouteau formation at Burlington, with the Louisiana limestone of Missouri, and to regard the fossils of the shale as constituting the oldest Kinderhook fauna.

Assuming the Grassy shale to be a northward extension of the southern Chattanooga black shale it may be accepted that the shallow, northward advancing seas in which it was deposited finally encountered the northwestern waters in which still swarmed Devonian types. The blue shales which preserve remains of the latter are known as the Lime Creek shales in northern Iowa, and the Snyder²⁷ shales in central Missouri. The Saverton green shale which immediately overlies the Grassy black shale with its first Carbonian fauna, appears to have been laid down in seas which marked a slight advance from the north. Whether or not the Saverton fauna is really a purely Devonian facies hanging over from a previous time, or whether it is really identical with the Snyder fauna is yet an undetermined question. The fossil assemblages now known in them are yet too meager for final determination; but present evidence points strongly in this direction.

²⁴ Iowa Geol. Surv., Vol. X, p. 69, 1900.

²⁵ *Ibid.*, Vol. III, p. 433, 1895.

²⁶ *Ibid.*, Vol. X, p. 70, 1900.

²⁷ As a formational title Snyder was long used as a field name by the Missouri Geological Survey. It was a name appearing upon the cadastral maps of Callaway county; and was widely known by the dwellers around Fulton. Its first appearance in print may have been by Galleher in 1900; but it was also defined clearly by Keyes in 1902. Between the date and the time when Greger proposed the title Craghead from the same section the term Snyder was used in a number of publications. Greger's name is therefore a synonym.

EDITORIAL

JUDICIAL ATTITUDE IN GEOLOGICAL CRITICISM

In these days of reconstruction and recovery from the intellectual stagnation initiated by a World War Science in every branch is sorely tried. With so many of the ordinary avenues for the free diffusion of knowledge blocked or closed channels yet open become unduly crowded. Under these conditions it does not seem to meet the spirit of scientific debate to have censorship on memoirs offered made unnecessarily rigid. If anything it should be widely loosened for the time being at least.

At best organized societies constitute the conservative force in Science; and only too often the venerable officers in charge tend to become ultraconservative, much to the detriment and usefulness of the organization. Loss of prestige on this account is usually speedy and is frequently complete before it is fully realized.

Censors on articles, and those of government bureaus particularly, are prone also to take themselves all too seriously. Something is radically wrong in the premises when a college professor, acting for the moment as scientific censor reports against publication of a geological paper, not for any reason of technical defect or shortcoming in thought, but merely because it happened to contain a few alleged grammatical gaucheries which elsewhere find countenance. In this particular instance the paper rejected proved to be really the most important scientific contribution of a decade. So effete had become "science" in one of our great universities. What consummate presumption must some nincompoop have to hold up under the guise of censorship for five years a geographical essay from the pen of such a high authority in his line as Prof. William M. Davis, as was done not so very long ago? And the "censor" was not left on Flanders Fields.

Even the late Prof. J. D. Dana, of the *American Journal of Science*, was not always on guard. Once he returned to the author, Dr. Gustav Hinrichs, a brief article on "Pangensis," which contained the clear, graphic and mathematical demonstration of the mutation of the metals, wherein it was shown that if a proper force could be found that would dislodge a certain number of atoms from the lead molecule another metal would be formed, and so on, until gold could be obtained. The philosopher's stone, the great quest of the ages, was at last discovered. It was half a century before chemists accepted the idea. Another time the distinguished editor turned down that great article on the "Kettle Moraine of the Second Glacial Epoch," because it was "local geology," thus depriving Professor Chamberlain of the full credit of setting forth first the establishment of the complexity of the Glacial Period — one of the half dozen great geological thoughts of the Nineteenth century. In both cases, long after publication in other channels, Dana wrote apologies for his negligence.

At the close of the discussions and on return from the long transcontinental excursions of the Twelfth International Geological Congress held in Canada, I wrote out a concise summary of opinions that had been expressed by the world's geologists on Pre-Cambrian sedimentation; and because of its especial biotic interest the concensus of opinions was sent in to the *American Naturalist* for publication. In the body of the article it was expressly stated that it was not a personal argument but a composite. For censorship the editor unwittingly entrusted the paper to one of his University colleagues, who, manifestly utterly unfamiliar with the subject matter himself, bumptuously opposed himself to all the chief authorities and workers on Pre-Cambrian problems in the world, and returned the following archaic expression:

I cannot see that the manuscript which you sent to me can be called in any way a contribution, nor does it even give a summary of current knowledge. It is an assumption of a lot of facts which have not been, in many cases, before stated and of course have not been proven. Many of the fossils which he calls Pre-Cambrian may turn out to be after all Lower Cambrian. This is not my own opinion alone but that of Professor Rothpletz and others as well. The author does not say anything about the fossils he names — not even the classes to which they belong — nor does he tell us anything about the history of their discovery. That sort of thing would be interesting to the general reader but I do not see that what

he has to say has any such interest. As for the final table which he calls "Known fossil zones of Pre-Cambrian Rocks" and his classification, that is, to say the least, pure speculation. The names he proposes have no standing, are not defined, nor is there any evidence that the order of superposition which he gives is the true one. Indeed, we know of some of his divisions that they do not have the position at all which he assigns to them. That table should surely not be published in the "Naturalist," with the implied impression that it has been approved by geologists. There is a very definite sub-division at the present time of the Pre-Cambrian which may be obtained in our [my] latest textbooks but that is very different from the one given. If he has any evidence for such a classification he ought to bring that out in a geological journal before he attempts to give this as a table for use of the general reader. If he would write a paper giving the history of the discovery of these early fossils and the present status of opinion as to their nature and age, it would be worth while, provided it were accurately done, to publish it in the "Naturalist," but I certainly think it would be a grave mistake to publish this paper as it stands and I would certainly advise against, in any case, publishing such a table as he gives.

This, despite the fact that the fossils mentioned incidentally were those described by Walcott, and the general geological section was essentially that of Lawson; both had described the several features in publications so well known that no ordinary working geologist would need footnote references. It turned out that this selfsame censor at that very time had ready to publish a college text-book on geology in which he had omitted all reference to two-thirds of the geological column, a time-span greater than that existing between the base of the Paleozoic sequence and the present. Small wonder is it that he had never heard of the article's contents before. But why should he be foisting upon unsuspecting young people statements purporting to be latest opinions, that had really passed into oblivion a hundred years ago. And why should any editor of any scientific journal need to seek that sort of advice.

The instance is not a solitary one. The majority of proceedings of our learned societies and our magazines are losing much of their worth just now by such discouragement of productive effort and are casting aside their most cherished heritage. The spirit of academic freedom of discussion begins to grow anemic and to wither away. Effects of World War penetrate every nook and corner of human endeavor. It requires herculean effort to stay the powers of atrophism.

One may well compare this wild outburst of intemperate, impolitic and unjustifiable buffoonry with those calm statements, conspicuous for their sane, judicial equipoise, of the late W J McGee, on passing on a submitted memoir of mine, which brief Dr. E. O. Hovey, Secretary of the Geological Society of America, afterwards sent to me. In his youth McGee practised law, which fact may account, in a measure, for some of the particularly judicial trend of his comments. The fair-mindedness and supreme disinterestedness of argument is only fully appreciated and admired when it is recalled that the entire paper was directed against the very things for which he himself had long contended, and destruction of his own most cherished theses was involved.

Doctor Keyes' paper, "Deflative Scheme of the Geographic Cycle in an Arid Climate," reached me duly, and opportunity has just been presented for reading it critically. Please find it herein, and I answer the conventional catagoric inquiries here rather than on the blank.

Taken in connection with Doctor Keyes' earlier papers on substantially the same theme, the scientific matter presented in this paper is original and of value—I incline to say of decided value.

The general arrangement of the matter is satisfactory, and the presentation neither over-full nor unduly concise; the graphic illustration is certainly not redundant, and I think might well be increased. Yet in this connection I have the feeling that the paper as a whole betrays either some haste in preparation or incompleteness in general coördination. It seems to me that as whole the paper is hardly thought out with that degree of fullness desirable in the summary expression of a series of papers designed to make a contribution to knowledge concerning a subject distinct from that now incorporated in text books, yet measurably parallel with the latter. I have the feeling that Doctor Keyes might with advantage to himself and with benefit to students re-write some of the matter in such manner as to bring out definitely parallels and contrasts evidently clear in his own mind yet not fully expressed in the manuscript. Perhaps on this point I do injustice to the author, and should be pleased if the manuscript were sent some other censor for judgment on this point.

The literary execution *per se* is good, so that the paper can be published with little more than formal editorial revision.

Accepting the scientific value of the contribution, I am moved to offer suggestions which might aid the author in a revision which seems to me desirable; these follow later.

Subject to revision with respect to the matters of fact considered in following paragraphs (and despite the question in my mind as to whether the matter is so fully thought out as might be desirable), I recommend the publication of the paper.

Despite full recognition of the originality of the modern idea of eolation brought out by Hill, Udden, Cross, and Free, but of which Keyes is the principal exponent, and while seeing in this idea a way to the explanation of phenomena otherwise obscure, I am more impressed by this paper than any other of Keyes' productions that in the enthusiasm of the new idea he underestimates the importance, and virtually denies the significance, of certain plain facts — chiefly the unmistakable evidence of water sculpture throughout the greater part of the region to which his discussion applies. To make my meaning clear, I note a few examples.

For two-thirds or three-quarters of the length of the Southern Pacific Railway between Deming and Yuma that railway traverses plains which in the rough way seem approximately horizontal, but which are always inclined and represent slopes conformable with (and evidently due to) the running water of the region. The fact that they conform with the running water is established by observation of residents and of critical travelers; but it is demonstrated beyond all peradventure by devices employed to protect the railway against sheetfloods, in which most of the running water of the region moves — for while the nature of the floods seems not to have been recognized by the engineers, it has been by the respective section-bosses who have worked out their own devices for protection — the most effective being extended wing-dams reaching from occasional culverts obliquely up-slope to finally meet with the corresponding wing-dam running up from the next culvert. In a broad way, more work has been required to construct these protective devices than to build the railway grade; and they attest the practical recognition on the part of railway attachés of the great fact that the broad plains traversed by the railway are not only water-shaped but are subject to overflow (tending to continue the shaping process) every year.

The approximately east-west divide between the streams flowing toward the Gila and those running directly toward the Mexican Gulf, nearly coincides with the international divide from Nogales westward; and there is a pronounced difference in topography on opposite sides of this divide, evidently due first to continental uplift producing a south-western tilting, and second to water sculpture as influenced by this tilting. North of the divide the intermont plains are comparatively smooth, whether of planed rocks (due to sheetflooding, in my judgment) or of alluvial accumulations in the central valleys. South of the divide the intermont plains are more deeply sculptured, and over something like three-quarters of the entire surface are molded into southwesterly sloping terraces or mesas, forming the most conspicuous feature of the average landscape. Many of the streams (albeit more or less ephemeral) have under the stimulation of the southwesterly tilting retrogressed entirely through transverse sierras; and after such retrogression they have continued working headward into the invaded intermont plain, developing a configuration conspicuously of the dendritic, wide-branching type due to water sculpture. The prettiest example known to me of this invasion of smooth

plain by retrogression of a river through a range is a few miles south of the international boundary, near the mining camp located by Professor Blake some years ago; it is some twenty or thirty miles west of the Mexican custom house at Sasabe. It was mapped in some detail for me by Willard Johnson, but unhappily his failure of health prevented completion and publication of the map. A somewhat more accessible example appears at Sasabe; there the retrogression has been through a general mountain mass rather than a distinct sierra, but the change in configuration from the Mexican custom house to the American custom house at Buena Vista (which is just on the intermont divide) is striking, while the great water-cut slopes extending southwestward are conspicuous in the country extending from Sasabe to Altar. The most conspicuous example of river retrogression through a range is a hundred miles further southward, where Rio Boccuache has cut a narrow gorge through a lofty sierra; its head drainage has deeply sculptured the intermont plain generally protected by that range; in the gorge the ground water is brought to the surface by the little pervious rocks, so that this part of the stream is perennial while both above and below it is commonly a mere sandwash. These examples might be multiplied; but it suffices to say that they are typical and express the general fact that fully two-thirds or three-quarters of the surface throughout the hundreds of thousands of square miles of desert known to me are manifestly shaped primarily by water sculpture — albeit generally by sheetfloods rather than by streams.

Throughout the region contemplated by Keyes, and as I have seen it, the conspicuous feature of the sierras is ruggedness; they not only rise sharply from the plains as he describes but rise deeply in cliffs, picachos, and minor and major crests, generally of bare rock, sometimes displaying the characteristic exfoliation forms of granite, but more commonly sculptured into the distinctive forms produced by water-action — indeed nowhere else do I know of mountains so eloquently attesting energetic storm-water work as those of the arid region. This is not an exceptional case, but the common one — indeed it is characteristic of the entire region as known to me.

Now in the light of these facts I can not help feeling that Keyes lays himself open to criticism and opens his conclusions to distrust, by virtually denying that the configuration of the arid region is explicable, save on the supposition of a predominant deflation. I think I see how his mind is influenced — perhaps obsessed — by the idea of deflation to such an extent that his mental pictures emphasize those facts and features of configuration which deflation explains; but with a view to his own protection against criticism no less than with a view to the correct interpretation of nature, it is desirable that he should guard his expressions more carefully and especially avoid denial of the existence of evident facts attesting water sculpture — facts which in their geographic extent are far greater than those attesting deflation.

Of course I recognize that Keyes through deflation supplies the missing

link in explanation of the topographic development of the arid region. My difficulty always arose in explaining what had become of the material removed from the one-time plateau now reduced to a few remnantal sierras, save on the hypothesis of filled-up canyons in the axes of the valleys, but Keyes satisfactorily explains the disposal of the debris through deflation. In general, too, his mental picture of the intermont rock-floors forming the greater part of the region in question and of the sharp rise of the rugged sierras from these floors is signally just—in which, by the way, he differs widely from Tolman (whom he quotes), whose illustrative sections reveal hypotheses having practically nothing to do with the facts of the region as known to Keyes and some others no less than myself; indeed when I had the satisfaction of meeting Tolman and pointing out to him that his great ideal aprons of colluvial material were really so tenuous as to be worn entirely through in the three-inch deep path leading up to the Tucson Desert Laboratory, he was entirely at a loss, and could only think of this as an exceptional case instead of the general one for the entire region. Now I have no doubt that Doctor Keyes will take in good part the foregoing remarks which, if critical, are constructively so rather than destructive; and that in the light of my experiences in the region which he describes, as set forth herein, he will be able to so modify his expressions so as not to lay himself open to less tolerant and friendly utterances by others. I do not specify passages in detail; for the important thing is for him to get the spirit of what I have said and apply it to each passage as he re-reads the manuscript—for, as you see, I think it should go back to him in the light of suggestions herein.

To my mind the use of the term “deflation” is questionable. It is quite true that it has been extensively used, having been put in current circulation in this country by Johannes Walther (first in a publication which I happened to edit), yet none the less it is so bad philologically and the same term is so clearly fixed in its philologically correct meaning that it really ought to be permitted to drop quietly into innocuous desuetude. I feel confident that if this point were put up to the author he would acquiesce in the judgment as to the unsuitability of the term.

Again, I am forcibly struck with a certain dearth of bibliographic reference. Of course, I realize that when the paper was first prepared it must have been designed rather as a preliminary announcement or a caveat on the field it covers, and that as the matter has grown in the author's hands he has somewhat modified the treatment without changing his viewpoint to what it would be were he to take the matter up anew to-day. In my judgment Doctor Keyes would find himself materially fortified in his position, and at the same time be able to increase materially the effectiveness of the presentation by some marshaling of current authority; and I do not think he would lose anything on the score of credit for originality. The bibliography of the subject is now in excellent condition, thanks to the work of Mr. Free, and it would be quite easy for Doctor Keyes to obtain any needed access to this material.

The foregoing expressions, as you will see, are predicated on the assumption that the material of both papers is appropriate for publication by the Geological Society of America; and of course they would not be made if I did not incline rather strongly to that judgment. At the same time I am fully aware that while such is my judgment, it would not be the judgment of a considerable proportion of the geologists of the country most likely to be called upon for opinion, perhaps it might not even be the judgment of a majority of the Fellows of the Society. The great fact is that Doctor Keyes is in his papers occupying what is for this country at least a nearly novel viewpoint; and from that viewpoint he has brought together rather in a broad and general than in any special or conclusive way an assemblage of phenomena of which the greater part have either been interpreted otherwise or have not been interpreted at all. His concept of the development of western America, and especially of the Great Basin, is one of great boldness, and one involving decided originality despite the recent contributions of Hill, Cross, and others in this country, to say nothing of Walther and numerous other students in other countries where the phenomena are seldom developed on anything like so grand a scale. Should Keyes' views prove valid they will in no small way mark a new stage in the growth of geology, a stage probably no less noteworthy than that marked, *e.g.*, by glacial geology. Now it is in this light that it seems to me the papers should be viewed rather than in any narrow and technical one; and without committing myself as to the validity of his views I am unqualifiedly of opinion that a man of Doctor Keyes's standing is entitled to the right of the tribunal. Then, when the question rises as to the tribunal, I find myself inclining strongly to the opinion that the Geological Society of America is *par excellence* the fitting one. Of course, if his interpretations do not stand the test of time there will be those who will feel that the Society somewhat lowered itself in publishing them; while on the other hand in case the views become current there will be those who will point with complacency, if not with pride, to their early exposition under the ægis of the Society—while in any event the Society will have to its credit a specific effort toward that promotion of knowledge which is one of its primary objects. Such are among the considerations leading to my judgment as to the propriety of publishing, whatsoever the future may bring forth.

Having for a number of years given a good deal of attention to the geologic processes emphasized by Keyes (which for a half-generation I have been calling "eolation") and having repeatedly worked in the arid region at intervals from 1881 until last year, I ought to have, and probably have, about as definite convictions as anyone else concerning the validity of Keyes's views. In this connection it is not needful to say more than this; on the one hand the prevailing notions as to the genesis of that area of half a million square miles which we may include in the Great Basin are so far from satisfactory as to cry aloud for revision or extension or some other mode of perfection; while on the other hand

the detail configuration throughout say 90% of the area has the character of waterwork rather than windwork. Now the windwork hypothesis would seem to explain the general features, albeit most of us are so set in our ways of thought as not to see this readily, but can hardly be made acceptable to most of us without some consistent explanation of the rarity of its ear-marks on the vast footslopes forming the greater part of the area of the Great Basin. Nevertheless, for one I am willing to give Keyes and Hill and Cross and Free and all the rest of the workers the fullest opportunity to work out details and improve inconsistencies, for while art is long, science is longer and must have not merely its day in court but its decade of re-trial. Another consideration also impresses me. Of late what is sometimes called "problem" geology is temporarily out of fashion, at least in this country; and I cannot help feeling that those of us who are predisposed toward the long look both backward and forward should seize every reasonable opportunity for encouraging the presentation of problems promising to advance the fundamentals of our knowledge. So again, in a word, I incline strongly toward giving Keyes the benefit of the doubt.

MOST PRODUCTIVE FIELD IN HISTORICAL GEOLOGY

On that most enjoyable evening which members of the Trans-continental Excursion from the Twelfth International Geological Congress spent at Field Station in the heart of the Canadian Rockies Doctor Walcott, having come down from his eyrie high up above timber-line on Mount Field, his famous "Burgess Camp," incidentally remarked in the course of his informal lecture that the Pre-Cambrian section of the West presented the most fruitful theme that today awaits the young and ambitious geologist. Recent novel conceptions and developments have greatly emphasized the cogent necessity and desirability for early and concerted action in unearthing life forms from the rocks lying beneath the Paleozoics which we now know.

An amazing circumstance connected with the consideration of the pre-Cambrian formations of this country is the notable absence of that strong and concerted effort towards exact correlation which so long has characterized the descriptions of the younger rocks. To be sure numerous local sections are constructed, but nowhere is there hint of their reduction to a definite scheme based upon time-equivalency.

In this regard general treatment of the pre-Cambrian rocks presents a very marked contrast with that of other geological terranes. In the final analysis their consideration and their map-

ping are really in strict accord with the old, discarded and untenable Wernerian principles. It is the dark corner in modern geology. It is a feature which holds over from a bye-gone age. A century and a half ago the correlative methods employed would have been eminently proper ; but today they are far out of fashion. Nowhere do we find them in keeping with modern schemes of terranal classification. As one field-worker astutely observes, after the elapse of half a century, the taxonomy of our pre-Cambrian rocks remains almost in the same chaotic state as when they were first made known.

Few of the International travellers just alluded to had ever seen so deeply into the oldest stratified rocks in so short a time, under such favorable circumstances, or under happier guidance. Some of the participants in these proceedings preëminent in other fields of stratigraphic endeavor, seemed to see in this old American complex an exact counterpart of conditions that were presented a century ago by the Primary (Paleozoic) masses when they were awaiting the magic touch of the English geologists to unfold the then inextricable maze.

Between the two-century-apart problems there is one marked difference. In America there appears to be in place of only one grand succession of formations two vast piles of eral rank, either one of which very greatly surpasses in magnitude and span of time the entire Paleozoic sequence with which Murchison, Sedgwick and Lonsdale had to deal. And still beyond lies an Eozoic Era which perhaps is forever hidden from prying eyes.

PALEONTOLOGICAL GEOLOGY

Anatomy of Early Trilobites. Out of the wealth of finely preserved trilobitic remains recently unearthed from the Burgess Shales of the Mid Cambrian section above timber line on Mount Field, in the southwestern part of British Columbia, there are a number of specimens which display the ventral appendages and other little understood structures of these organisms.

The appendages of these animals are preserved almost with the same distinctness as those of the crustaceans of modern seas. One of the Burgess shales forms is Rominger's *Neolenus serratus*.

This trilobite had a singularly thin test. As compared with that of the common King crab of modern waters it had about the same thickness in individuals of like size. The test of the axial and pleural lobes of *Neolenus* was reinforced by rounded ridges and local thickenings that gave it notably increased strength. Connection by muscles with the ventral integument gave it a rigidity that would withstand relatively great strains without flexing or breakage. With the muscles of the coxopodite extending through the ventral integument to the strong axial process the base of the limb had firm support.

The animal could thus use its legs to walk clear of the seabottom, or to push its way through the soft bottom mud and sand as it searched for food; or it could sink or emerge from the bottom ooze very much as does the *Limulus* of today.

One structure is of especial significance. Presence of epipodites on the limbs of *Neolenus* being challenged special examination of this feature was instituted. Their existence was corroborated by Messrs. Ulrich, Ruedemann and Bassler.

"The surface of the epipodites exhibits no trace of the transverse inosculating lines which are generally present on the exopodites, being so far as these wrinkles are concerned, entirely smooth

under magnification. On the other hand, certain structures are rather clearly indicated on the epipodites that are wholly wanting on the exopodites. Most important of these is a line of denser substance running some distance within and parallel to the margin of both lobes. On close inspection small denticles are observed projecting from one side of this inner line. From these and other corroborating facts observed it is inferred that both surfaces of the epipodites bore two spiniferous carinæ which united on the smaller lobe. Except at these carinæ the walls of the epipodites seem to have been exceedingly thin or at least more tenuous than those of the exopodites. On account of their isolated and exposed position, not being held together like the exopodites by long overlapping fringes of setæ, and lying between the endopodites and outside the exopodites, they were much more liable to be lost."

WALCOTT.

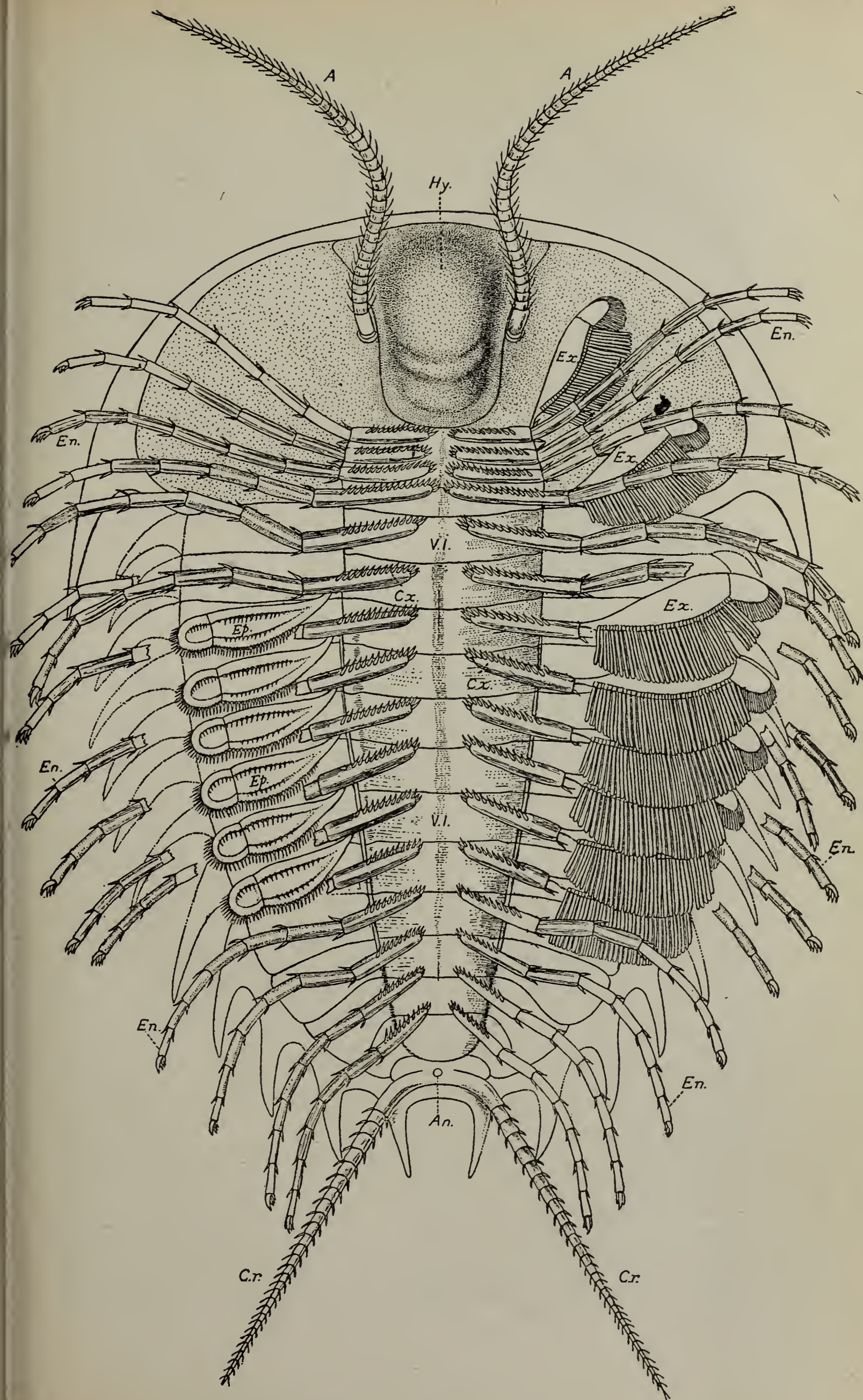
Extinction of the Tetracoralla. In a paper by Professor Raymond, in a recent issue of the American Journal of Science it is suggested that the extinction of the Tetracoralla at the end of Paleozoic time was the result of a chilling of oceanic waters by the melting of the Permian Ice-sheet.

In order to consider this idea quantitatively the following assumptions must be made: (1) That the Paleozoic reef-building and solitary Tetracaralla had a habit similar to the present reef corals as to depth and temperature, the modern reef-builders being confined to shallow tropical and sub-tropical waters; (2) that the volume of the ocean has remained constant from Permian times to the present, except as it was altered by glaciation and deglaciation; and (3) that the continental areas, including the submerged continental shelves, have been relatively constant from Permian to Present Time.

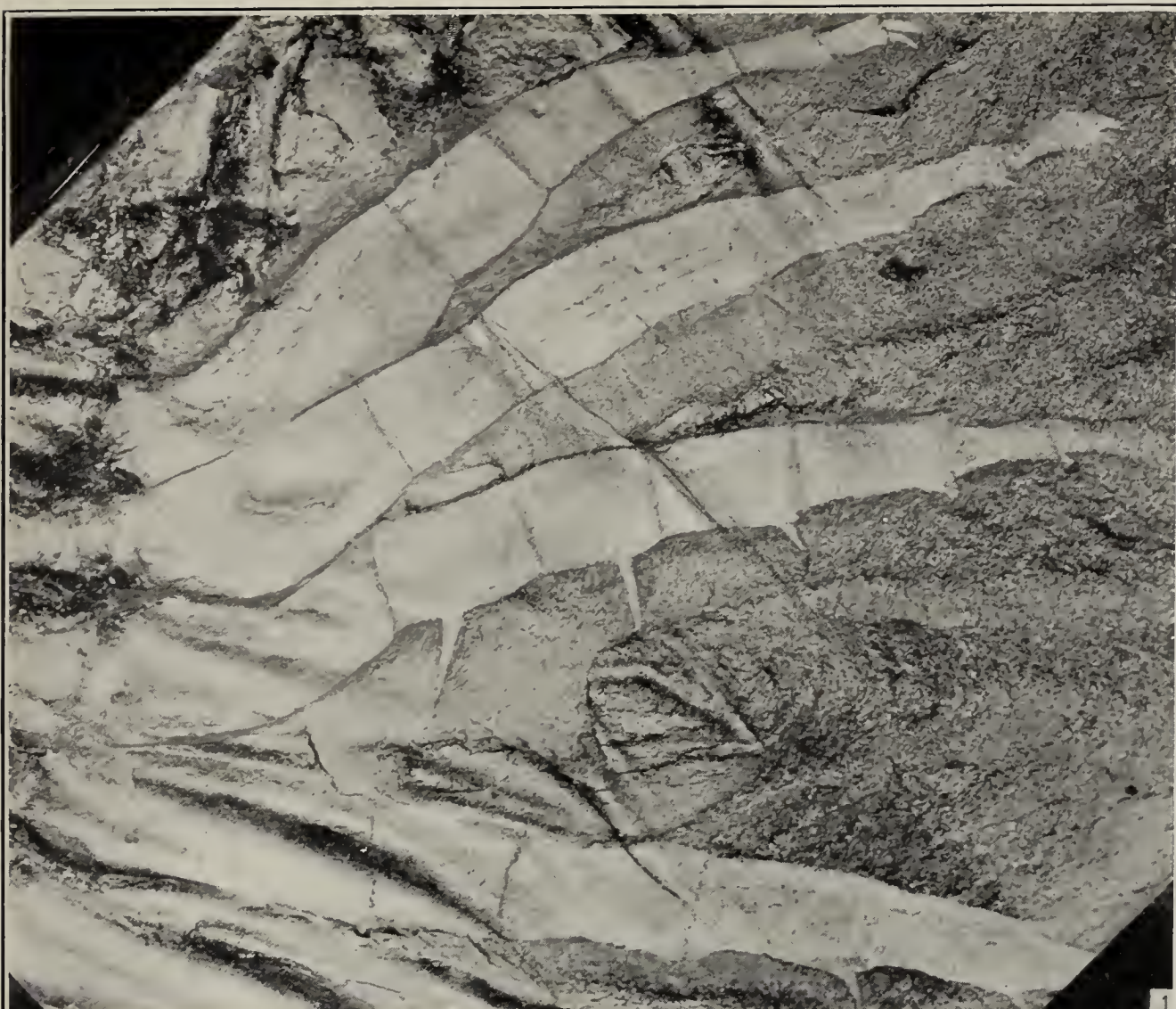
Taking the volume of the oceans as 1,300,000,000 cubic kilometers, according to Murray and Hjort,¹ a change of one degree centigrade would give off or absorb 1.30 times 10 to the 24th power calories, assuming that a cubic centimeter of water weighs a gram and the specific heat of water is one calorie.² Even in these days of oceanographic investigation it is difficult to get an average

¹ Depths of the Ocean, p. 210.

² Water does not weigh exactly 1 gram per CC at all temperatures from zero to 100° C, nor is the specific heat exactly one, nor is it constant from zero to 100° C; but these approximations are close enough for the calculations in this paper.



ANATOMY OF EARLY TRILOBITES



1



2

STRUCTURES OF EARLY TRILOBITES

temperature for the oceans; so that it would be hazardous to guess at an average temperature of the Permian ocean.

If it is assumed that Permian glaciation withdrew sufficient water from the oceans to lower the surface 600 feet over its entire area, granting the area to have been 139,235,000 square miles (the same as the present area) this amount of lowering would be ten times that estimated by Daly (50 to 60 feet) for the Pleistocene glaciation. The volume abstracted to form ice would be 17,412,00 cubic miles or about five and one-half per cent of the total volume; that is, approximately one-twentieth of the total volume. The latent heat of ice is 80 calories per gram or 5.39×10^{24} calories. If all the heat for melting this ice came from the ocean then the temperature of the ocean would be lowered $5.20 \times 10^{24} \div 1.24 \times 10^{24}$ which equals 42 degrees. Inasmuch as the remaining nineteen-twentieths of the ocean was above zero the mixing of 19 volumes with one volume at zero would result in a lowering of .05 for each degree centigrade of difference of temperature, probably making in all less than 5 degrees centigrade.

If the Permian glaciation withdrew from the ocean an amount of water only equal to that withdrawn by the Pleistocene glaciation this figure of temperature change would be .5 degrees centigrade. These figures are based on the ocean supplying all the heat necessary to thaw the ice, during which time the ocean neither received nor radiated heat energy. It is obvious from the areal distribution, as well as the character of the deposits of continental ice-sheets, that very little of the ice could have melted in contact with ocean waters, and where this was the case the sea was not the sole source of heat. In other words probably 95 per cent of the ice-sheet melted in contact with either the atmosphere or the land, and the resulting water flowed through rivers of greater or less length to the sea. The lowest temperature of this water could not have been below zero centigrade, the equilibrium temperature of ice and water.

If we accept Clark's estimate of the volume of the ocean at 302,000,000 cubic miles, which is somewhat smaller than Murray-Hjort's estimate, and the annual discharge of all rivers into the oceans as 6524 cubic miles, it would take about 44750 years to replace the entire volume of water in the oceans. However,

during such times as the melting of the continental ice-sheet the rate of discharge may have been much higher, possibly twice as much, in which case the time involved would be only half this figure. Just what were the temperatures of the oceans and the rivers is unknown; but even if all the water entering the oceans was glacial melt, which we know was far from being the case, the annual lowering of the average temperature of the oceans could only be 1-22375 of difference in degrees centigrade per year. As the area of unglaciated land during Permian glaciation was much greater than the glaciated area, and did not have a severe climate, the run off from this unglaciated area of large volumes of water at relatively high temperatures would offset to a large degree the cold water from the melting ice-sheet, and the actual lowering of the ocean temperature would be of considerably smaller magnitude, possibly only 1-50,000 or 1-100,000 of the difference in degrees centigrade.

It has also been assumed for the purpose of calculation that the oceans neither absorbed nor radiated heat energy during the time that colder waters from melting glaciers were being received. Inasmuch as the sun was radiating heat continuously during this period such assumption cannot be true of the oceans, and still less so of the inflowing rivers. As soon as the oceanic temperature was lowered the rate of evaporation was lowered and a portion of the radiant energy of the sun, formerly utilized for evaporation, would be available for warming. This factor would constantly oppose the lowering of temperature due to the influx of cold water and slow down or counterbalance this cooling.

Aside from the foregoing calculations it may be remarked that the best opinion as to the age of Permian glaciation places it in the early part of the period. If this be the true age then the *Tetracoralla* survived this event along with the rest of the floras and faunas, just as so many organisms survived the Pleistocene glaciation.

Conclusions are: (1) That in the maximum of the first case there could be a drop of 5 degrees, which does not appear to be competent to lower the oceanic waters of the entire globe sufficiently to exterminate all of the *Tetracoralla*; in the minimum of the first case, it is absolutely unimportant biologically. (2) When we consider the second case it is seen that under the most favor-

able circumstances it is not probable that the lowering of temperature ever amounted to very much, and certainly not enough to be critical for the Tetracoralla.

GEORGE M. HALL.

Lowering of Life's Record into the Abyss of Time. Speaking from the viewpoint of a morphologist the late Professor W. K. Brooks once made a striking statement that one standing on the brink of Cambrian seas could get no better prospect of life that had gone before than one does today. Despite the necessary inference that the paleontologist need not hope to ever unearth fossils essentially different from those which he now knows from the Cambrian rocks and that these really approximate the forms which existed when life first became established on the bottom of the sea pre-Cambrian fossils will yet be long sought.

Since Brooks' observation of 30 years ago pre-Cambrian remains of organisms have come to light in abundance. Both in the Belt Mountains of Montana, and in the Lake Superior region pre-Cambrian forms are now known. In past attempt to discover fossils in strata older than those of Paleozoic age a most serious obstacle to success always is the highly altered condition of the ancient rocks whenever they are exposed to view. The well-known geologic law that the older a rock is the more metamorphosed is it likely to be especially applies to the pre-Cambrian formations. It is a criterion of such great weight that it is still a decisive factor in the determination of the relative ages of these old rocks. In the majority of cases known metamorphic processes have gone on so long and so intensely that it is often almost impossible to tell whether a rock-mass was originally igneous or sedimentary in character.

Great significance must be attached to the establishment of the antiquity of the oldest fossil fauna as indicated by the finding of the organic remains in the pre-Cambrian Marquettean strata of the Steep-Rock Lake district. At a single step these fossils carry back the record of life on our planet beyond the middle of Archeozoic time, or a chronologic distance nearly twice as long as all Paleozoic time. Even the fossil horizons of the Beltian section, according to the best calculations, are well towards the base of the Proterozoic succession, the next eral span behind the Paleozoic

section, or as far behind Cambrian time as the latter is from the Present.

In onerous attempt to carry back farther than Cambrian time the geologic record of life on our globe progress, through a period of more than two generations, seemed so inappreciable that many students of ancient organic remains almost despaired of ever seeing their efforts in this direction rewarded. At no stage, however, during these long years was the problem actually entirely without hope of solution. Latterly there was rapid accumulation of pertinent facts. So suggestive were some of them that an eminent English geologist only a decade ago was led to predict with no little confidence the final differentiation of the pre-Cambrian complex into orderly succession of formations not very unlike that of the familiar Paleozoics. Results of the past ten or two without warning more than fulfilled the most sanguine expectations.

The wide interest aroused by these recent discoveries of abundant well-preserved organic remains in rocks of unquestionable pre-Cambrian age is secondary only to the enthusiasm produced a short while ago by the actual location of the fossiliferous horizons in the general geological column. As definitely determined these oldest fossil-bearing levels are stratigraphically more than two miles beneath all other previously known horizons yielding traces of life. These revelations are, of course, as important biologically as geologically. They materially modify all our previously held views on the subject. They open up to us a more inviting field of investigation than awaited the paleontologists of the first half of the last century when they started to unravel the life record anterior to Cretaceous time. They promise even greater triumphs than when the Paleozoics first revealed their inscrutable secrets to Murchison, Sedgwick and Lonsdale.

We fast approach that nethermost level so remote that we may not expect to find hard parts of organisms, because not yet formed in the course of organic evolution. Thus we have already almost reached a point beyond which paleontologist can not go, beyond which he must seek in vain for further treasure, and beyond which the paleontological vesta is forever hidden from the human eye.

KEYES.

Occurrence of Oldest Known Trilobites. The Cambric sections of western America have an extraordinary development. In the Silver Peak district, beyond Tonopah, the Early Cambric strata aggregate more than 6,000 feet. This Silver Peak section is especially instructive because of the fact that it furnishes remains of the most ancient trilobites of which we have knowledge. Two forms in particular characterize this low horizon which is within 200 feet of the base of the section exposed.

The oldest trilobitic zone is designated the zone of the *Nevadia weeksi* fauna. The species represented are *Nevadia weeksi*, Walcott, and *Holmia rowei*, Walcott. These forms continue far below the *Mesonacis gilberti* fauna which belongs to the upper part of the Early Cambric formation. There are no other trilobites known to occur in the 5,000 feet of strata in which they are found except the Mesonacidean genera *Nevadia*, *Holmia* and *Olenellus*. *Ptychoparia* is represented by a single fragment somewhere in the uppermost 400 feet of the section.

The two species mentioned are all that have been discovered thus far in the lower beds. A fragment of perhaps an *Olenellus* occurs 1000 feet above. The form known as *Olenellus claytoni* comes from an horizon 3000 feet above.

A noteworthy feature connected with this primitive fauna is that there are no traces of forms having a large pygidium. Early stages of growth of the young of all of the Mesonacidae represented have a very large cephalon but a very small pygidium. Both of the trilobites described are characterized by minute pygidia. *Nevadia weeksi* has a large cephalon, 28 thoracic segments, and a very small plate-like pygidium, without a defined segment. *Holmia rowei* also has a large cephalon, 16 thoracic segments, and a very small pygidium which has one distinct segment.

It is sometimes stated that the primitive trilobite was a flat, free-swimming form with subequal cephalon and pygidium. If this were so it would seem that forms with large pygidia should occur in great variety and numbers in the earliest Cambric faunas. These oldest forms do not sustain this contention. *Microdiscus* and *Eodiscus* occur at a much later stage in the Early Cambric sequence.

WALCOTT.

Lilley and Devonian Fishes. This note records the death of Albert Tell Lilley, a local collector of Le Roy, Bradford County, Pennsylvania, who did much in the course of a long life to make known the fossil fishes of the Chemung and Catskill rocks of his native state. His demise occurred at Sayre, Pennsylvania, on February 12, 1922, at the ripe age of 84 years.

When studying the Devonian sections of northern Pennsylvania twenty-odd years ago the writer met Mr. Lilley and visited with him the outcrops where the latter had found many of the types of the Bradford County fishes described by Professor Newberry in Monograph 16, U. S. G. S. One of these forms Professor Newberry named in his honour *Sphenophorus lilleyi*.

Lilley had eyes to see and imagination to restore the life represented by the fossils abounding in his native hills and they meant as much to his life and happiness as did the birds of Selborne to Gilbert White. Lilley was not satisfied with bringing the Devonian fossils of Bradford County to the notice of paleontologists alone, but took pains to initiate the boys and girls of his home town into the mysteries of the rocks. He was for many years a school teacher, and in the later years of his life he was connected with the schools of his township in other capacities. In a letter received from him just after last Christmas he tells of his success in stimulating the interest of boys in school work by occasional talks on geology. His work as a stone-mason during the summer seasons, between school sessions, gave him, as it did Hugh Miller, rare opportunities to discover and collect the fossil fishes of the Devonian rocks of his district. Many of his fossils are deposited in the collections of the Bradford County Historical Society.

Lilley's home was located on the edge of a post-Glacial gorge exposing one of the finest sections of the Chemung rocks in northern Pennsylvania. In this fact we have, perhaps, an example of an environmental control of the development of a local collector. This section was first brought to the notice of geologists by Mr. Lilley in the Proceedings of the American Philosophical Society for 1886. Lilley continued to collect from this and other sections nearby up to the very last year of his life. A letter sent the writer shortly before his death listed the species which in recent years he had added to the fauna known from the Gulf Brook gorge. After

collecting from this section for more than forty years he wrote with characteristic modesty, "I have added much to Gulf Brook section, but there is much more to add."

Lilley came from an old New England family, one which is in no danger of the extinction which appears to threaten some of the old colonial families, if we may judge from the information given the writer in the letter written at Christmas time. At that time Mr. Lilley assumed responsibility for 55 descendants,—9 children, 27 grandchildren, and 19 great-grandchildren.

Beginning his education in a backwoods school of Pennsylvania, Lilley learned to write with a goosequill pen dipped in witch-hazel juice extracted in an iron kettle. He commenced teaching in the same schools at the age of 18. In his genealogy of the Lilley and Smith families he states that reading the "rocky pages handed down from long ago" has been "the source of more pleasure than can be explained to the uninitiated."

To paleontologists the following quotation from Professor Newberry's monograph on the "Paleozoic Fishes of North America" will serve as a suitable and worthy token of the value of the contributes to Paleontology which resulted from the work of this tireless collector, who still delighted in the companionship of hammer and chisel in his 84th year: "Mr. A. T. Lilley, of Le Roy, Bradford County, Pennsylvania, has found many fish remains in the Chemung Group near his place of residence, and among them the representatives of several new genera and species, of which brief descriptions are given on the succeeding pages."

KINDLE.

Lunar Pertrifactions. With the increasing high-powers of the modern telescope, it will soon be time for someone to announce the discovery of fossils on the Moon. The names *Lunaceras* and *Trilunabite* are hereby reserved for that occasion. If the discovery be authoritative, a discussion of lunar inhabitants concealed on the off-side of that globe will follow, as a matter of course; and the door to controversy, closed just lately by a well written article on "Life in Other Worlds," in *Science* (Vol. LIV, p. 329), will again be open. Of course it may be necessary to demonstrate that life on the Moon has been the real cause of the volcanic phenomena visible on the near side of it. To anyone who may say that such

is impossible, let me answer that I, too, used to think that way. For minds that are not biased, there is ample room for discussion on that subject.

First of all, let us recall the fact that geologically we have not yet accounted for the presence of so many large mountain features on the Earth, as a suggestion, that we should not be too hasty in concluding as to the basic causes for the mountains on the Moon. The reason that we cannot yet account quantitatively for the origin of all the mountain structures here is probably, because (on the principle that great results come generally from a convergence of causes) some one of the major causes has been overlooked. The omitted factor is probably that of life as a geologic process in the making of mountains. That living things compose one of the great geologic processes is of course well enough known, but we do not seem to have given magnitude enough to it, in our theories, as to cause and effect in relation to mountain-building. The suggestion that life on the Earth may be the first cause of the building of mountains here, if not on the Moon, may seem a little startling, but is, I believe, well worth thinking about seriously, though not too seriously, of course.

The great obstacle to a full and clear consideration of the relations of organic forces to the origin of mountain ranges of the Earth and the other planets, is the theory of spontaneous generation of first life — a theory to which zoologists are wed, as nicely shown in the excellently written article already cited. It is not the intention now to protest against any atheistic, or mechanistic, hypothesis that may hold that inspiration in man, along with the life of animals and plants, has originated from an immensely complex concatenation of physica-chemical circumstances only, except to say, that it must stand on its own merit. The mechanistic view that life arose from matter that was not living, but became living at some fictitious time in the past, if accepted dogmatically, cuts out other important considerations. For example, it assumes that life began, which we really do not know yet, and that it is the consequence of things of which it may be really the cause — such as mountains. Accordingly I feel free to consider it as not probably true, as follows.

Living things, as we see them now and in the geologic record, are morphologically a unit group so that it is not necessary to as-

sume that life originated on the Earth more than once. Living things are also environmentally a unit. — Considering, then, the origin of protoplasmic life on Earth, the idea of spontaneous generation from inorganic matter is precluded by now existing circumstances, geologically. Life centres, as to environmental conditions, around the *niveau* where earth, water and air meet, and it ranges out towards the extremes of physical conditions but not to the extremities of it, — not into the volcano's fire, nor the glacier's ice, nor the outer air, nor the ocean's middle depth nor the solid earth's interior. Life on the earth occupies a common-place, or median, position on the Earth's surface, and also with relation to geologic processes. It evidently always has, and, since no condition of the earth appears possible to be mentioned which existed once that does not exist now, then if life arose *ab initio* from inorganic particles once it must have continued to do so even until now, for geologic reasons. Logical conclusion is that life is accordingly as old as the planet is, or else that it arrived from somewhere.

Further, so long as Pasteur's demonstration holds true, that spontaneous generation does not take place on Earth now, and we have no geologic reason for thinking that there was any better chance in the past for such an event, some other hypothesis appears called for. At any rate, there is need of a geologic vitalistic theory for the origin of protoplasmic life on the Earth.

Such a vital geologic theory may proceed simply from the circumstances that living things compose one of the major geologic processes. Life aids in the building up of islands and the filling of the seas. Further, also, organisms are intimately dependent upon the geometrically unsymmetrical condition of the solid earth by which it is pushed up in part through the hydrosphere into the atmosphere. If the lithosphere of the Earth were a perfect spheroid, we understand, with the ocean and the air equally deep and uniform around it, there could be none of the now living plants on it — and hence of course none of the animals. They are wholly adapted to the Earth's unsymmetry, and they do in fact help to maintain it. Only the simplest of all protoplasmic life could exist without it. The origin of living things and the origin of the geometrical unsymmetry of the lithosphere are accordingly with reason to be considered as closely related.

A vitalistic hypothesis for the origin of life, as protoplasmic organisms on the Earth, might be stated about as follows: The Earth at one time became perfectly symmetrical, geometrically, so that the sea and air, each spread over it in uniformity. On the surface of the sea, by combination of vital element and material elements, a single variety of protoplasmic life arose *ab initio*, in uniformity. By willful migration to the sunny equatorial zone, and, then by the precipitation of lime from the sea-water, some shallows and islands were built up. Those chalky masses, by weight, as it increased, disturbed the isostatic condition of the lithosphere, and thus started those diastrophic movements and volcanic outbursts that have built up the mountains and lands, dividing the seas, and in short making those physical differences on which the traced evolution of life known to us has so largely depended. It may be said, too, that the obvious difficulty for us, to define characteristics of a vital element when *not* in combination with matter, need not deter us because we have the combination with material elements in the traced evolution as well as in all living things, to define it.

Whether such a vitalistic theory is exactly true or not need not be stressed. It expresses more adequately and seems more truly the relation of living things to the earth and it to them than biologists are accustomed to do. The very intimate relation of living things on the Earth; to the earth's geometrical asymmetry is so obvious to a paleontologist that he might further suspect, very reasonably, some vitalistic significance in a general asymmetrical feature to the typically symmetrical material universe, as a whole — in case he happens to be looking that way. The mountains on the Moon suggest fossils whether they are there or not.

It seems to me nearly equally difficult to understand how the Moon can have either volcanoes or organic life unless there is or was water and air there, since the volcanoes of the Earth are said to be essentially steam-explosions. Until the quantitative relations of living things to both mountains and volcanos of the Earth have been calculated back to their time of origin, it may be too soon to say what the life on the Moon is or has been. In the meanwhile it may be fair, however, to keep an open mind as to the possibility of such things as *Trilunabite* and *Lunaceras*.

SARDESON

MINING GEOLOGY

America's Mountain of Gold. With that remarkable movement of gold across the Atlantic into the United States after the beginning of the Great War this country suddenly found itself the money center of the world. After the entrance of America into the great struggle European nations discerned a rapid reversal of usual trade conditions. They fully expected America soon to lose much of its wealth prestige that it had so quickly acquired. Never before in the history of mankind had there accumulated in a single country so vast a store of flowing gold. Probably never again will there be repetition.

Since the recent flow of the world's gold to the Occident set in thoughtful minds direct their attention not only to the probable distribution of the world's money supplies during the next few years, but to the natural reserves of the precious metals which each warring nation is in possession. At the present time the world's annual production of gold is not far from half a billion dollars. Of this huge amount the United States furnishes something more than one-fifth, with a material increase from year to year.

Introduction into the mining practice of the seeking and exploiting large bodies of low-grade ore instead of, as formerly, small veins of high-grade ore, witnessed a great expansion of the output of the principal metals. At the same time actual and potential ore reserves are enormously augmented. In the case of copper and lead this has already transpired. It seems soon also to obtain for other leading minerals. Gold in particular is especially susceptible to the new practice. For instance, a few years ago when a practicable method was devised to treat the sands of deserts, which often contain considerable amounts of

native gold, newspapers made much of the possibility of a gold deluge. To those who are most familiar with this golden carpet of the desert the increase of world output from this source alone promises to assume appalling proportions.

Although it is not probable that all of the rich gold deposits are yet exploited it is quite likely that not many very notable new ones remain to be discovered. The great supply of the future appears bound to come from the large, but low-grade, deposits which, because of hitherto lack of adequate methods of handling them, escape attention. The desert tracts of earth promise best results. There is still another and newer phase of the problem. This is the extraction of the gold content from the igneous rocks themselves, since they seem, in many cases, to be the original sources of the metallic salts. Not all igneous rocks appear to yield a definite metallic content. Those masses which are known to geologists as laccoliths thus far afford the best returns. In this respect laccoliths have a very great advantage over other igneous masses because of the fact that they are intruded magmas—peculiar bodies which have thick overburdens of strata that, as cooling and consolidation take place, prevent the metallic vapors from escaping into the air and being lost.

A notable laccolith, or group of these masses, is the Sierra del Oro in southwestern United States, not very far from the Mexican boundary. The early Spanish settlers of the region gave these eminences the title of Gold Mountains because of the fact that around their flanks existed extensive and very rich placers. In that day the name was applied to many localities. It has long since vanished from our maps. In pioneer days large quantities of native gold were obtained from this place. Being situated in a great desert the water supply was severely limited; and working was onerous in the extreme. Since a method of "dry-washing," or wind treatment, was devised operations on a large scale now seem perfectly feasible.

A new interest now awakens in the Gold Mountains. It turns out that the rich gold placers, of which they are the center, are only a relatively small part of their metallic wealth. As demonstrated by numerous assays the mountain mass itself is shot through and through with gold. Two of these mountain peaks alone show a gold content of more than fifty billions of dollars,

that is removable in the same way as the disseminated copper at the great camps of Bingham, Chino, and Inspiration. This amount is equivalent to the entire present world output of gold for a period of one hundred years. It is the production of this country at the existing great rate for more than five centuries.

For more than a quarter of a century the problem of the primary source of the metallic ores has formed the most prolix, many-sided and bitterest controversy that has ever been recorded in scientific annals. Out of this seeming futile and academic debate emerges what may prove to be the greatest and most successful mining venture of all history. That the close of this discussion should come so soon after the great World War, that it is really a direct consequence of the war, and that, in the economy of nations, it appears as far-reaching in its potency as any effect on the battle-field tending to eliminate world dominion by any one people, are themes wholly unexpected. Yet, commercially, nations may suddenly and entirely have to get off that gold basis which has been so long, so universally and religiously regarded as the foundation stone of all sound finance and, indeed, of civilization itself.

KEYES.

Origin of East Mesabi Magnetic Ores. The iron ores of the eastern part of the Mesabi Range are mainly magnetite. Since this magnetite has been altered in a few places to the ferric iron, producing secondary hematite ore-bodies, it is probable that that magnetite may have been the chief ore mineral all along the Range before weathering.

Peculiarities of the bedded and banded structures, the mineralogic composition, and the normal and probable chemical transformations seem clearly to indicate a primitive sedimentary origin of the ore beds. These are features which are so plainly manifest on the Mesabi Range that they give clue to the process involved in other districts where the several factors are more or less obscured.

It is now well known that cherts and the several iron-bearing layers are commonly precipitated from solution from ordinary phreatic waters and form beds of notable extent. Solution of the silica of the cherts may have been facilitated by the presence

of alkalis; but the solution of the iron would be more likely aided by the presence of acids. If carbonate minerals were the more abundant, as they certainly were in some of the other ranges, an alkaline bi-carbonate solution might have acted as a common solvent. Such solution obtains sometimes in connection with volcanic activities. In the present instance some alkali might have been derived from the great granite intrusion of the neighborhood.

As to the primitive source of the metal-content of unique, thick, and extensive iron-bearing formations like those of the Lake Superior region Van Hise and Leith appeal directly and mainly to magnatic emanations from the contemporaneous basic igneous rocks. Repeated alternation of ore material in the beds as they now occur seems to be a significant fact bearing upon the history of the iron formation. There are hundreds of alternations of fine magnetite and coarse fragmental layers. Rhythmic sedimentation is manifestly due in some cases to a rhythmic supply of differing materials. In a broad way, the supply may have been ferruginous at one time, slaty at another, and cherty at another. If the materials, however, had been derived from volcanic sources, as sometimes suggested, it appears improbable that supplies of iron oxide and silica could alternate so many times and on such a minute scale as the sediments indicate. It is unlikely that there could have been so many successive lava flows. Volcanic rhythms should produce alternations on a large scale. Furthermore, climatic rhythms are also large features. Alternation of layers from one-tenth of an inch to six inches is better attributable to seasonal or other occasional changes in conditions. And these changes would affect a chemically depositing sediment only in shallow water. Since it is believed that the pebbles were formed in shallow water the inference is that standing water acted upon the ferruginous chert, enriching it much as the certain ores of the Lake Superior region are being enriched at the present day; by removal of the silica mainly, but only to small extent by the deposition of metal in its place. The suggestion here made as to the origin of the pure magnetite in no way conflicts with the possibility advanced by Van Hise and Leith, that some oxides may have been precipitated directly in very pure form.

It can hardly be assumed without qualification, however, that

primary deposition and reworking of the iron resulted in the production of magnetite. Precipitation of this mineral in water at ordinary temperatures is not a reaction commonly observed in nature. There is reason to believe that the precipitate was a chert, with ferrous silica, and ferrous carbonate and more or less limonite. Oxidation and formation of intraformational conglomerate probably produced limonite. Change from these minerals to magnetite is a sort of transformation that may take place in several ways. Ferrous iron of the silicate and carbonate and of the more oxidized limonite reacts directly to form magnetite. If the oxidation be very complete a reducing agent is readily available in the organic matter which must have been present in certain beds. These and other reactions have been sufficient, during the long ages of recrystallization under conditions of heat and great pressure, to transform most of the iron oxide into the mineralogical form of magnetite. This process is not touched upon by either Van Hise or Leith.

In summation, deposition probably occurred in shallow water by precipitation, mainly as an organic process, resulting in lean ferruginous cherts, with more or less siderite, ferric oxide, and greenalite. Alternating with periods of precipitation came periods of solution, leaching, oxidation and wave-action, producing intraformational conglomerates, granular rocks much richer in iron, and doubtless some layers of pure ferric oxide.

The richer deposits of magnetite are so characteristically in the granule and conglomerate zones that it is possible that the primary leaching was a determining factor in the development of the richer magnetites from the lean ferruginous cherts, greenalite rock, etc. Later, when covered by other beds there may have been more or less concretionary rearrangement. Deep burial under the slates developed heat and pressure which recrystallized much of the formation. The iron minerals reacted at this time with one another and with organic matter, and probably with other reducing or oxidizing agents, thus producing the magnetic oxides of iron. Recrystallization produced shrinkage cracks, and regional tectonic movements developed folds but appear to have had no appreciable effects on the concentration of the iron.

GROUT.

First Mention of the Ores Zinc in America. Notwithstanding the fact that zinc is the last of the common metals to come under the complete control of mankind and that as an element, the date of its recognition is scarcely 200 years back, some form or other of it, as an earth of peculiar composition, is known to have been in use in the arts for nearly forty centuries. The circumstance that Greek coinage dating 1000 to 1500 years, B. C., contains a definite proportion, so large as 23 to 25 per cent of zinc indicates clearly that it was at this remote time utilized in alloy. In this country the zinc industry is so recent in origin that its beginnings are still well within the memory of men living yet the existence of the metal appears to have been very early known.

In North America the mention of zinc for the first time now appears to be almost if not quite as early as that of lead. Unlike the case of the latter metal zinc was never sought for ammunition, the provision of which was so vital a problem to trapper and pioneer in the New World and hence there did not exist the great demand for it. The earliest mention of the ore or metal is usually regarded as that of John Bradbury, an officer who, investigating the resources of this country in the interests of England, traveled, in 1810, through the Louisiana Purchase country, as the region west of the Mississippi river was then called. In the same year zinc was described and analysed from the Franklin Furnace.

There appears, however, to be a distinct reference to an American occurrence of zinc very much earlier than any other heretofore specifically noted. In 1655 a French adventurer in the service of England, Pierre Radisson by name, and his brother-in-law, Médard Grosielliers, visited the Indian tribes dwelling in the neighborhood of what is now Dubuque, Iowa, and spent a season mainly on Iowa soil.

In the course of his account of the resources of the region Radisson quaintly says that "In their country are mines of copper, pewter and lead. There are mountains covered with a kind of stone that is transparent and tender and like that of Venice." On the whole his description is remarkably lucid. The mention of pewter undoubtedly refers to zinc ore. It will be remembered that this term is the old English title for spelter

(English pewter, Dutch piauter, Dutch and German spiauter), and that the name was appended to both the alloy and the ore.

That Radisson's reference does not allude to any other metal than zinc is conclusively shown by a number of circumstances. Dry bone is the common associate of the galena ore of this deposit, and it would be easily recognized as the "pewter ore" of England with which the explorer must have been acquainted. In Colonial days pewter-plate was an important possession of the best households; and the finding of the substance at the mines naturally made a profound impression on an active mind — an excitement second only to that of a gold discovery.

At the time of Radisson's sojourn at the Dubuque locality the mining of lead had already developed into a considerable industry. The mineral had indeed at this time been taken out during a period of more than two decades — ever since the famous visit of Jean Nicolet, in 1634, who in the interests of the fur-trade had introduced fire-arms among the Indians, and with them created an active demand for ammunition. A main reason for Nicolet's turning back at this point rather than going on in his great quest of the South Sea and a short route to China as he had set out to do, may have been this very discovery of the lead deposits as an unlimited supply for bullets.

Of the three widely separated localities in which lead was first mined in this country — previous to 1650, the Dubuque field is the only one so far as is now known in which any zinc ore also occurs. That zinc should be thus early recognized so long before it was actually used on a large scale elsewhere is a fact of exceptional interest.

KEYES.

Location of Wisconsin Road Metal. Fully one-half of the cost of improved highways is spent for the materials and their transportation to the place of construction. Large savings are effected, (1) by location of deposits of material near the project, thus saving cost of transportation, (2) by location of a usable type of material in a region thought to be without good road material, and (3) by location of good deposits which may be bought more cheaply than those previously available.

In general a survey is made on each side of the proposed high-

way construction for a distance of at least as great as that from the nearest railroad station (a source of rail-hauled material). The quantity and quality of material available are reported upon in considerable detail. For concrete pavements this involves screen analyses, as well as silt and colorimetric tests, of all gravel and sand deposits both developed and undeveloped. Quarries and rock-outcrops, as potential sources of crushed rock, are given detailed study. One division engineer reported that such a survey saved the county \$30,000.00 on one project alone. In this case the saving was entirely due to reduction in cost of truck haul.

For surfacing, a search is made for all available material, whether this be gravel, shale, limestone, granite, clay, or sand. Not only are all available exposures examined, but in the case of undeveloped deposits, test-pits are dug. After elimination of all but a few of the more desirable deposits, the engineer carries on more extensive exploration of these before determining which particular deposit should be developed. Mapping on a more detailed scale than is possible on published maps is necessary as an area too small to appear on such a map may be underlaid by sufficient material for a large project.

This work of course requires geologists of considerable field experience, since it involves not merely the making of geological observations, such as would be necessary in a glaciated region to distinguish terminal moraine, ground moraine and outwash. He must determine in what particular knoll of the terminal moraine, or in what small tract of the outwash plain, will be found the best kind of road material. This might be accomplished by tedious and expensive test-pitting over a whole outwash plain, for example, but a properly qualified geologist through his knowledge of the minute details of glacial geology should be able to eliminate large areas from consideration, and concentrate his efforts on the most favorable ones. In the study of an esker, instead of having to make test-pits throughout its length, the geologist should be able to select at a glance the places most likely to yield that particular kind of material needed for the project under consideration. In the area of glacial Lake Wisconsin, the soils are generally sandy. A search is made for places where the underlying lake clays are under only a thin sand-covering. Such a search involves a careful study of topography

and soils in order to locate small areas where the clay is readily accessible.

In the non-glacial areas of Cambrian sandstones the detailed stratigraphic sections already published prove of great practical value, since in this area deposits of Cambrian shale are the only material available for surfacing the sand roads. With this stratigraphic knowledge as a basis, the geologist studying road materials must select the particular sites where the material needed can be secured and delivered on the road with the least possible expense. In driftless southwestern Wisconsin, which lacks the wealth of readily accessible road material to be found in much of the glaciated area, search is made for limestone of suitable quality and availability. The sandstones are studied to find a formation suitable for fine aggregate for concrete, or as surfacing for heavy clay soils. Stream-gravels are located for use as surfacing and for concrete aggregate.

In the areas of thin drift overlying granitic rocks, the glacial material is not of much value for road material. Search must therefore be made for disintegrated granite suited for road surfacing, or for fresh granite which can be crushed for concrete.

Investigations of this kind are especially valuable to the Highway Commission, because such surveys save money; valuable to the geologists because they add greatly to geological knowledge of the State. They help the geological profession, since these surveys have convinced a large number of people that geology is practical and that by its application large amounts of money may be saved.

BEAN.

New Borate Field in Nevada. Announcement of the "recent" find of important borate deposits in Callville Wash, in southeastern Nevada, is only one of the many illustrations of the supreme complacency with which Government bureaus attempt to impress the public with some valid *raison d'être*. Borax mining is singularly free from scientific dependence. Although the famous 20-mule team advertisement is largely fiction, and finds greatest activity in traveling around the grounds of the St. Louis World's Fair, its very existence prevents the normal exploration of the Death Valley deposits.

After the discovery of borate crystal in extensive beds, in the

form of calcium borate, or the mineral Colemanite, in California, and the subsequent abandonment of the process of extracting boric acid from lake waters, the industry concentrated around Daggett, a small station in the Mojave desert. Death Valley deposits were too far away from rail to be profitably mined; and the 120 miles which 20-mule team traversed were confined, it is said, to a single trip.

In the meantime the famous colemanite deposits of Lang, north of Los Angeles, were opened up, giving by far the best transportation facilities of any. This was far back in 1907. At the same time new and extensive borate deposits were discovered in Death Valley; and the borate-bearing beds of southeastern Nevada — the recent new discoveries — were also then pointed out. But the Lang deposits proved so extensive and so near market that all operations elsewhere were severely curtailed or shut down.

After fifteen years of continuous operation the Lang deposits began to show signs of exhaustion; and preparations are now under way to develop the next best field, which is in the Muddy Mountains district of southeastern Nevada, now served by the Los Angeles and Salt Lake railroad. The extent to which private enterprise provides for the future, and the curious lethargy displayed by ponderous Government bureau in making great and sensational mineral discoveries a decade and a half after private corporations only too cogently indicate the need of a closer adjustment of results if the mineral industry is to receive adequate benefit. Although reviewing the borax activities year after year for two decades the only noteworthy result obtained, apparently, is a sumptuous monograph setting forth a grand theory of origin, which, however, falls flat before the first breath of boreal wind.

KEYES.

Potash Wells in Western Texas. Summarizing the evidence recently obtained showing the existence of potash salts in Texas the following borings are noted.

In 1912 S. M. Swenson's borings at Spur, in Dickens County, gave a brine at 2,200 feet below the surface, which contained 5.4 per cent of potassium, calculated as chloride.

In 1915 a boring at Boden, in Potter County, furnished some

red salt, probably polyhalite, occurring somewhere between 875 and 925 feet below the surface, and this red salt contained 9.2 per cent of potash, calculated as oxide.

In the same year the Miller boring, in Potter County, furnished somewhere between 1,500 and 1,700 feet below the surface, some red salt that contained 6.1 per cent of potash calculated as oxide; and at some level below 1,700 feet it had some colorless salt associated with anhydrite. This salt contained 10.5 per cent of potash, calculated as oxide.

During 1921 there was found in the Bryant boring, in Midland County, in cuttings accumulated while boring from 2,405 to 2,425 feet below the surface, a mixture of colorless and red salt, shale and anhydrite, yielding 6.9 per cent of potash, calculated as oxide.

In the early part of the spring of 1921 the La Mesa Oil & Gas Co.'s. Burns No. 1 boring, in Dawson County, at a depth of from 1,864 to 1,865 feet below the surface, disclosed a red salt which yielded 10.8 per cent of potassium oxide.

In the Means well, in Loving County, about 40 miles north of Pecos, samples show 15.5 per cent of potassium oxide, coming from a depth of 1,000 feet, and there is 8 per cent of potassium oxide in samples from between 1,855 and 1,860 feet.

In the River well of the A. Pitts Oil Co., about 8 miles east of Barstow, in Ward County, near the Pecos River, showings of 14.4 per cent of potassium oxide were obtained from samples taken at 1,600 feet. In a sample taken at a depth of 1,875 feet in the same boring there was found 10.5 per cent of potassium oxide.

In the G. A. Jones et al. Long well, in the southeast part of Borden County, an analysis shows the presence of 22.9 per cent of potassium oxide in a picked sample from between 1,070 and 1,075 feet; and a picked sample from cutting between 1,075 and 1,083 feet shows 17.68 per cent of potassium oxide. The sample from 1,115 feet, similarly picked, shows 6.59 per cent.

UDDEN.

Recovery of Low-grade Magnetitic Ores in North Carolina. Success of recent concentration tests upon the low-grade iron ores of Ashe and Avery counties promises important economic developments at an early day. Among other problems taken up by the

North Carolina Geological Survey during the past year relating to the iron reserves of the state was the question of improved ore treatment, the hope being that large bodies of iron ore now undeveloped because of their location might be made available for market.

As described by Dr. W. S. Bayley, who lately undertook for the Survey a special inquiry into the genetic relations of the ores in question the large open-cut walls of the Cranberry Mine present most excellent illustrations of the several phases of vein-filling and their relations one to another. "The magnetite is closely associated with the pegmatite. The miners declare that the richest ore is always near the pegmatite. Pegmatite and magnetite veins both cut the lean ore, which is a mixture of hornblende and magnetite, and magnetite impregnations extend from the walls of the magnetite veins into the bordering rock, causing an enrichment of these, and giving rise to magnetite forms a constituent of coarse pegmatite, exactly as does feldspar, quartz, and hornblende. It has the same shape as the other components, and the individual grains, when not aggregated, are of the same sizes as the grains of quartz, hornblende and feldspar.

"More frequently the magnetite forms groups, either alone or with hornblende, and these constitute lenses in the pegmatite. There is a strong tendency for the hornblende and magnetite to occur together. They appear to be the last components to separate, and often they occur in great masses forming lean ore deposits. Of the two perhaps magnetite is the later, since veins of this mineral penetrate the lean ore. It is probable, however, that the magnetite separated at two stages, of which one was contemporaneous, or nearly so, with the great mass of hornblende, and the other was distinctly later. When the two minerals occur together in the lenses the hornblende is apt to occur on their borders with the magnetite in their centers; and when arms extend into the surrounding quartz-feldspar mass the main portions of the lenses may be composed of magnetite or a mixture of magnetite and hornblende, while the extensions consist entirely of hornblende.

"There seems to be no question but that the ore of the Cranberry deposit was made before the date (Jura-Trias) assigned to the intrusion of the Bakersville gabbro."

Samples of the Cranberry low-grade ores were treated at the Bureau of Mines Experiment Station, at Minneapolis, where special tests were made on the possibilities of concentrating these ores. These results seem to indicate that a satisfactory method of concentration may be readily devised. By dry method a good smelting product, assaying better than 56 per cent iron and only 1 per cent phosphorus may be obtained through concentrating two and one-half tons of ore into one.

A vast new tonnage of iron ore is made available.

PRATT.

Circulatory Cycles of Ore-bearing Waters. Movement of ore-bearing groundwaters is not a simple sinking of meteoric waters into the earth's crust, but a complex and composite mingling of currents from many sources. On the theory of the meteoric agglomeration theory of the origin of the earth, the original and often the immediate source of ore-materials can hardly be in nature so largely magmatic as it is vadose. Qualified in some ways and strengthened in others, the general arguments of Forchhammer, Sandberger, Winslow, Van Hise and Bain assume a new interest and an added value. The main shortcoming, if such it really be, is merely in ascribing a sole, or principal, origin of the ore-materials to rock-weathering, when a somewhat broader interpretation of the facts seems necessary.

Ore deposits in the main are precipitated from aqueous solutions. Solution, transportation and deposition of ore-metals are distinctly processes operating through the medium of subterranean waters. The sources of the ore-minerals, the courses which they follow through the geologic formations, and the immediate causes of their localization, are factors of prime importance in the consideration of ore-genesis. Obscure as are the migration and changes of ore-materials it is possible, as will be seen presently, to represent graphically their general courses through the earth.

The various phases of primary ore-genesis may all be reduced to four principal groups: (1) extraction from sea-water; (2) inclusion of metallic minerals as accessories in the igneous rocks themselves and the subsequent liberation and segregation of the ore-materials through weathering-processes; (3) production of metalliferous bodies in connection with rock-masses in a molten

state, either through magmatic secretion or by expulsion of the volatile compounds of the metals during the progress of magma-cooling; and (4) derivation of metallic particles from extra-terrestrial sources, and their later segregation through the action of percolating surface-waters. Of these several groups contention regarding the first mentioned is now obsolete; the conceptions concerning the second and third enter into nearly all of the recent discussions of the subject; the idea of the last receives yet only incidental attention, but it is likely to prove the most important of all.

It is not difficult to fancy the manner in which metallic substances of meteoric origin may become incorporated with ore-materials generally. After reaching the surface of the earth, both cosmic dust and the larger meteorites must mingle with the soil, more or less quickly oxidize, and enter, by means of the circulating groundwaters, or otherwise, the deep-seated zone, in the same way as any of the heavier mineral particles liberated from the surface rocks through decomposition are supposed to do. The processes involved are essentially the same as for the changes and movements of rock-forming minerals. The distinction to be made is that, instead of the ore-materials being derived from the breaking down of the rocks of the lithosphere, a very large proportion is regarded as coming from extra-terrestrial sources.

Although there is probably no such universal sea of groundwater as that pictured by Van Hise, there is yet no reason for believing that surface-water readily penetrates to the deep-region, even to the zone of rock-flowage. The lithosphere thus represents merely the flotsam and jetsam of the globe, through which the heavier materials may migrate, generally inward as individual particles, but occasionally or spasmodically outward, in connection with volcanic flows.

In the course of the inward migration of ore-materials temporary ore-bodies are often localized, in the vadose zone, chiefly. How much of these materials are of recent extra-terrestrial origin and what proportion is the product of rock-decay, is at the present time difficult to estimate. The meteoric contribution has received as yet insufficient attention. That it may be more important than has been suspected hitherto is clearly shown by recent observations in desert regions. That this is the main source of vadose ore-

materials now seems not unlikely. It is probable that most of the diffused metallic content of the sedimentary rocks is in reality immediately derived from meteoric sources; for its derivation from the country-rock of mining districts, especially in tracts far removed from volcanic activity, has never been a very satisfactory explanation.

As commonly regarded a mineral vein consists of (1) the deeper primary portion below groundwater level, and (2) above the latter a limited weathered part known as the gossan. The lower part is composed of sulphides and the upper portion of oxides. Of late, between the two, at groundwater level, there has come to be recognized a third zone, that of secondary sulphide enrichment. According to this simple conception the movement of ore-

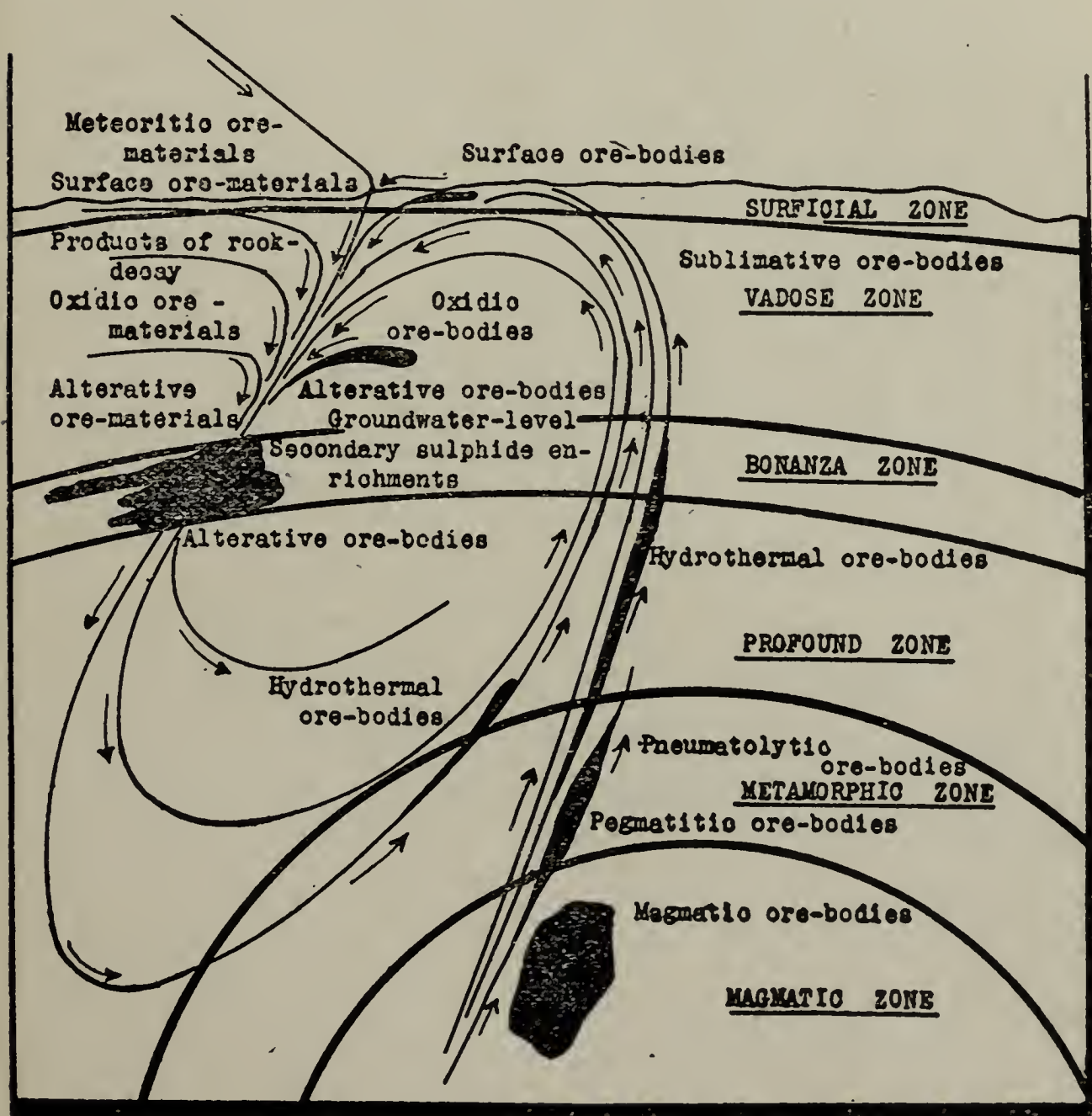


Fig. 19. Cycle of Ground-water Circulation.

materials liberated by weathering and passed into solution is merely slowly down the course of the vein.

The circulation of ore-matter seems to be very much more complex than this and the immediate sources of the ore-materials are widely different from a mere settling down along a vein. The ordinary circulation of metalliferous groundwaters is graphically indicated by the subjoined cut (Fig. 19).

In the diagram the courses of the various circulations are represented as merging at a single point or along a given line. The latter may be an old mineral vein or it may be a recently formed fault-plane. In either case the currents reach ground-water-level where their burdens of oxidic ores are mainly reduced to sulphidic form, and dropped, forming the bonanza zone or layer of secondary sulphide enrichment. A minor part goes on downward into the profound zone. The proportions of metallic substances derived from each source are not easy at this time to accurately evaluate. Meteoric sources perhaps supply much larger amounts than it has been customary to suppose. The part liberated by secular decay of rock-masses is probably the largest. By the oxidation of small masses of sulphide ores there is an appreciable contribution. Through the constant working over of the bonanza layer the ores are kept localized and concentrated.

At any rate, deposition of ores derived from the vadose is very much more important than is usually premised.

KEYES.

World's Oil Reserves. From a strictly commercial angle the future of petroleum is a theme of transcendent attractiveness. It is the most momentous single problem in the industrial activities of our century. In the United States, which, since rock-oil came into general use, furnishes two-thirds of all the oil yet won from the ground, the possible early limitation of supply finds universal solicitude.

Notwithstanding the fact that American fields may now be regarded as producing over sixty per cent of the world's annual yield, yet the United States alone is consuming more than seventy-five per cent of this total output. At present the yearly requirements of the United States are not far from 600 millions of

barrels. This is nearly 125 millions of barrels more than the country produces. World perspective is vital.

At the recent engineering congress on the petroleum industry, held in Kansas City, the subject of oil reserves came in for wrapt attention. Doctor White's remarks were most pertinent. Whether or not his estimates were entirely too conservative as many believed is not at point now. His figure for the total oil possibilities in the world was placed at seventy billions of barrels. Granting that this estimate is much too small, it furnishes a good basis for later and closer calculations. His further figures were forty-three billions of barrels for the amount found in regions of earth in which oil has been already proved to exist in commercial quantities.

With the United States supplying two-thirds of the yearly output, the other one-third is furnished by Mexico, Argentina, Bolivia, Colombia, and Venezuela, South Russia, Eastern Siberia, Persia, Mesopotamia, Assyria, Arabia, India, China, Japan, and the East Indies. The developed oil districts of these widely separated regions are undoubtedly a very insignificant part of the total possible fields that will eventually be brought into market. Geological oil discovery is yet only in its infancy.

"In the United States we have at the present moment produced a total of five and one-half billion barrels of oil. We have, therefore, used up more than one-third of our estimated original heritage of oil. Contrasted with our more than five per cent rate of annual depletion, the rest of the world withdrew in 1921 not much over 280 millions from its store of over sixty billion barrels, or less than one-half of one per cent of its reserve recoverable by present methods. In other words, the reserves of the rest of the world would stand the present rate of drain for over two centuries. From the standpoint of the world distribution of oil and the economic relations of our reserves to those of the rest of the world, an error of two or three billion barrels — four to six years' supply — in the estimate of the oil reserves of the United States, is comparatively insignificant.

"The coöperative committee expressly affirms that not all the oil-pools in the United States will have been discovered a generation hence. On the other hand, the committee takes great pains

to point out that though our reserves would not meet our present rate of consumption demands for twenty years if they could be taken out of the ground fast and cheaply enough to supply our market (a conditioning clause that has generally been lost sight of by the press and the public), this country will be producing oil for as long as seventy-five years to come."

In reckoning available oil supplies the enormous potential yield of oil shales of some of the western states should not be forgotten. These may produce ten times as much oil as remains in the ground in other parts of the United States.

KEYES

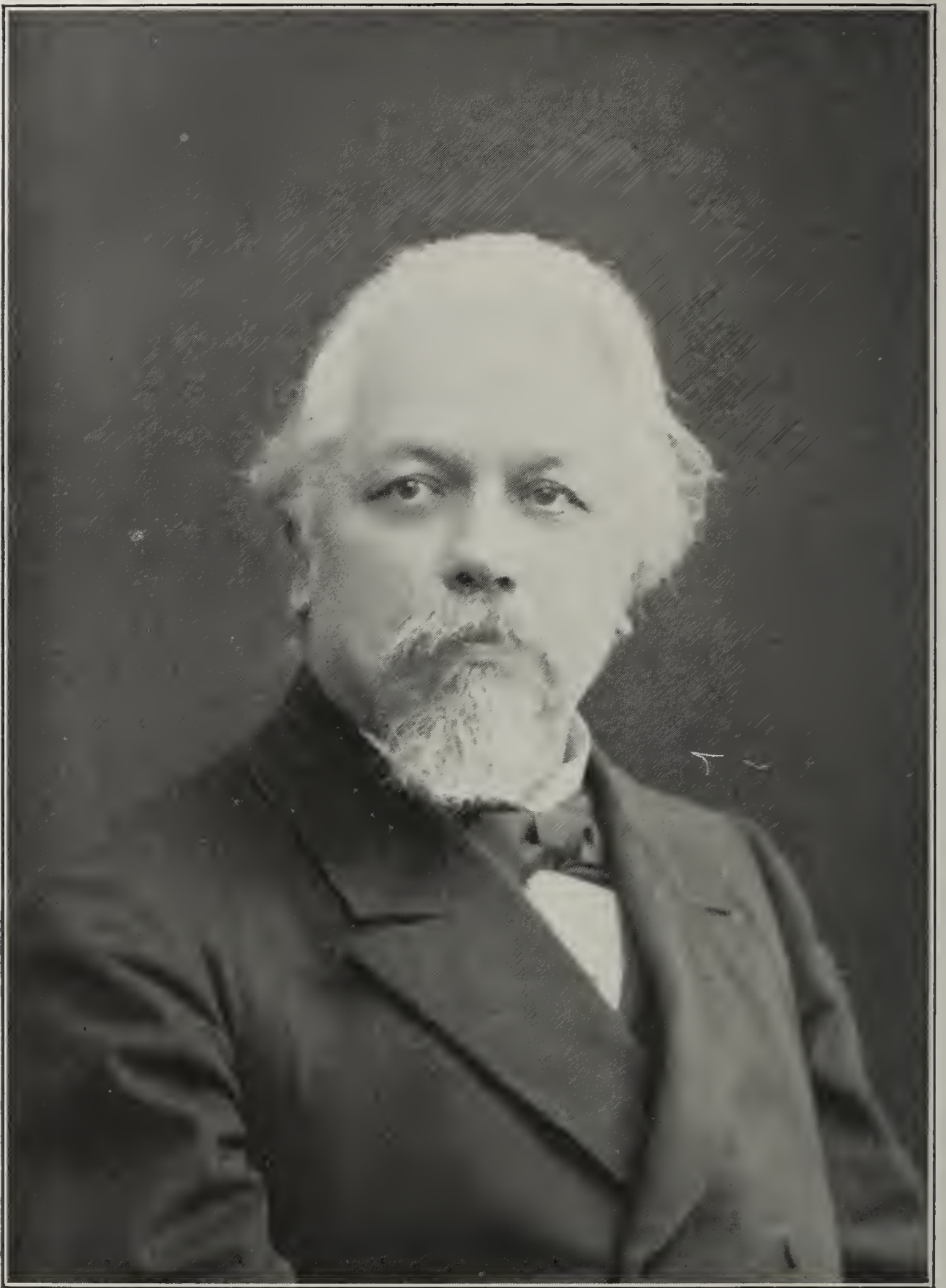
Verity of the Pipe-Vein. Recent controversy on the theme of whether or not there actually exists a pipe-vein type of ore-body recalls to mind an inspection, some years ago, of a unique silver deposit on the San Augustine plains a few miles from the foot of the lofty Sierra Magdalena in central New Mexico. If ever there be ore deposit answering to such description as pipe-vein this one surely appears to fill the bill. Although its rarity and small importance are fully recognized the occurrence itself is of no little interest as indicating that according to the most approved modern classification of ore deposits it should find a place.

"Float" samples of very rich silver glance were discovered by some prospectors. After considerable searching over a large area this float was traced to a low butte, now called Silver Hill, where after a little digging the country-rock was disclosed with the ore encased. The rock was apparently a granodiorite, or porphyry as the miners more commonly term it. The ore-spot was opened up and the material assayed, the results giving extraordinarily high values in silver.

The "ore-body" was found to be oval in cross-section, about 12 by 18 inches in size, but ran straight downwards. A shaft was begun and the ore-pipe followed to a depth of 340 feet. At this depth, according to reports, water stopped further sinking because of lack of suitable pumping machinery, and also for reason of the fact that the ore lead began to flatten greatly. According to all accounts this ore-pipe was associated with no other ore-deposits of any kind, nor with any fissures.

A part of the pipe assayed as high as 3600 ounces of silver to the ton; and 20 per cent copper.

KEYES



Harpsing

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RIGHTFUL DEMESNE OF PETROLOGY¹

BY PROF. CHARLES P. BERKEY

Columbia University

Petrographers are often so occupied with the task of describing and classifying so many historically unrelated things that the workers who come after the leaders in their science frequently entirely miss the true function of their efforts not only in their educational but their scientific bearings. In attempt to point out what this rightful sphere is it is not so very far wrong to assert that Petrology is the science of rocks; but in saying so we do not make ourselves entirely clear. To the average student of this subject who must judge from what he is taught and from what he finds in the texts, petrology appears to be a highly complicated lot of methods of descrimination, coupled with still more intricate schemes of classification, by means of which he finally finds some mystifying name for a very common and innocent looking fragment of rock. This is hardly real petrology. It is hardly beyond the most elemental stage.

After the first steps in a new science are taken it is not sufficient to assume that a long list of strange names, descriptive terms, and tabulations of fact can satisfy the demands of an inquiring mind. Petrology is just entering upon a more advance stage than that mentioned. This is evidenced often in the growing impa-

¹ Abstract of paper read before the Geological Society of America at the Chicago meeting.

tience with which are received the rock names based upon compositional differences. The chief positive influence comes from the practical use made of petrologic methods by the various workers in Applied Geology, rather than from the transcendent results of scientist pure and simple.

In the past our leading petrographers lent the weight of their influence and the fruits of their labors to field of tabulated details and of the niceties of systematic arrangement, as if the chief aim were to divide and subdivide and divide again, and to discover still more subtle and intricate methods or bases for further subdivision. Formal description was recognized as a more or less necessary accomplishment, but this commonly dealt with the features useful in the scheme of classification being followed, or else it relaxed into a rambling account of everything in sight.

Thus, ready ability to describe and to classify rocks especially marked an accomplished petrographer. To discover in a rock some slightly different mineralogic proportion and to be thoughtful enough to give such rock a new name established one as an active and original contributor to the science. Some of the methods made use of have even gone so far as to completely destroy the actual rock before beginning the task of fitting it into its proper niche in the accepted classification. Sometimes there were established by this method constituent factors that the live rock never had, while there was overlooked or neglected other much more important features which it did have.

In stating these facts one does not desire to be understood as attempting to belittle this or any other pioneer work. It is simply for the purpose of laying foundations for a revised prospect by emphasizing the severe limitations of current practice in some places.

Much as one must appreciate and value the genuine contributions which have been made by the systematic petrographer, and by the chemical petrographer, through the nice points of discrimination and the increased appreciation of sound chemistry and the rigid requirements of quantitative thinking, and much as one must also continue to use the same methods of discrimination, the conviction is overpowering that the apparent object in view is not a sufficiently high goal. This is not the chief or most promising field of rock study.

Nor can there be such a thing as a science of Petrology wholly apart from the great science of Geology. Whatever of vital significance attaches to a rock is gathered to it from the consideration of the processes, and the agents, and the forces, and the materials of its geological setting and from its own history. Virtually all of its own contribution, its meaning to be read, returns to Geology again to enrich the knowledge of that great field, the reserves of geological history.

In so far as nice discrimination and true sense of proportion serve in detecting evidence of former conditions, or processes, or changes, or sources, they play directly to the new aim. Of course it is patent that such methods of investigation and such fine distinctions made with this intimate dependence upon close and accurate observation have immense disciplinary and training value quite apart from any particular aim of the science itself. For its highest *motif* discrimination for discrimination's sake alone is not enough. Quality, and meaning, and interpretation, and life history are higher objectives.

The successive steps in the determination of the origin and the succeeding modifications which an ore-body, or a vein matter, or a residuary product undergo are as properly a petrographic theme as is the like history of an igneous rock or of a sedimental terrane. Whether a rock undergoes much alteration since the time of its original formation, or whether much of its present make-up is added to it since its initial appearance, or whether it records some peculiar reversal in the course of its development along which it first started, may be infinitely more important geologically than its name, or its mineral composition, or an elaborate description of it.

Work for many years over a rather wide range of materials and problems convinces me that the real aim of petrologic study is practicability, and that the true sphere of the science of Petrology is in some way always involved in unraveling the mode of origin and course of the life history of the rocks. It is, then, a grievous mistake to be content with too narrow a prospect. For this kind of petrology a rock needs revised definition, a character sufficiently representative of some structural unit to justify distinct consideration in all of its petrologic, structural, economic and geographic phases, besides correlative treatment, so that a working

understanding of its life history and the genetic meaning of the physical unit to which it belongs may be acquired.

The proper mission of Petrology is, then, to reveal that life history of rocks. Applied Petrology must have for its chief aim the discovery of the meaning of particular rocks in terms of some special requirement. Any method, any criterion, or any nomenclature that serves this end is a part of the legitimate equipment of the earnest petrologist. Yet such investigator must be before all a broadly grounded geologist, or he cannot expect to register in this class.

OLDEST KNOWN PECCARY FROM AMERICA

BY HAROLD J. COOK

Agate, Nebraska

About nine miles north of Crawford, Nebraska, there was recently found in the lowermost clays of the Chadron formation, and immediately above the "Rusty" member of the Pierre shales, a pair of lower jaws of a peccary, which proves to be a new species. Since this is the first known appearance of any form of this family in America, it seems worth while to record the find at this time.

When the specimen was discovered, the writer was just leaving the field, and was not prepared with materials to collect fragile fossils. But as this specimen was partly weathered out, already badly cracked, and in a place where it would soon be destroyed, it was removed from the rock with a pocket knife, and carried back to town, wrapped merely in a pocket handkerchief. As a result of having to use such primitive methods, the incisors were lost, and the back portions of the jaws were so badly broken that



Fig. 20. Jaw of Peccary, *Perchoerus minor*, Cook

they are still in fragments. However, by care and patience, both mandibular rami with the last five cheek teeth, are restored without distortion, and are connected in position at the symphysis.

Perchoerus minor, Sp. Nov.

The type of this species is specimen No. HC458, of the Cook Collections.

An interesting condition is observed in the lower incisors. They are extremely crowded. The first and second are of about equal size, as indicated by their alveoli, and the third somewhat smaller. The alveoli for the first incisors are situated not only forward, but the full width of the teeth below and outside of the second incisors, which practically rest against the large canine. $1\bar{3}$ is situated outside and *below* $1\bar{2}$, on the lower front side of the canine, so that the three incisor roots form an equilateral triangle with each other. This is an extreme case of tooth crowding, without reduction.

The canines are relatively large. The first premolar is small and single-rooted. The second premolar is evidently of fairly good size. $P\bar{3}$, is a simple primitive suilline tooth, with a single high thin, sharp crown, and vestigial heel. $P\bar{4}$, while considerably worn, had much the same construction, but widened posteriorly, with a broad heel against the first molar. $M\bar{1}$ is considerably worn down, but is a very simple tooth. $M\bar{2}$ is slightly larger, the four principal cusps, being arranged in essentially two low cross ridges, with little complicated enamel folding. $M\bar{3}$ is long and narrow, and even more simple than $M\bar{2}$, and with a low, simple heel, or third lobe.

This species has very brachyodont teeth, and is most like Leidy's *P. probus*, but is smaller, and more simple dentition. It may well be ancestral to that species.

Measurements

Length of dental series, $1\bar{1}$ to $M\bar{3}$, inclusive.....	85 mm.
Length of dental series, $P\bar{3}$ to $M\bar{3}$, inclusive.....	56 mm.
Depth of jaw below $M\bar{1}$	23 mm.
$P\bar{1}$, anteroposterior (approx) 4 mm.; Transverse.....	—
$P\bar{2}$, anteroposterior (approx) 7 mm.; Transverse.....	—
$P\bar{3}$, anteroposterior 9 mm.; Transverse.....	4 mm.
$P\bar{4}$, anteroposterior 9 mm.; Transverse.....	6 mm.
$M\bar{1}$, anteroposterior 11 mm.; Transverse.....	8 mm.
$M\bar{2}$, anteroposterior 12 mm.; Transverse.....	9 mm.
$M\bar{3}$, anteroposterior 15 mm.; Transverse.....	7 mm.

SUMMIT PLAIN OF THE COLORADO ROCKIES

BY CHARLES KEYES

Of four great and relatively lately formed peneplains, which especially characterize the southern Rocky Mountain uplift, the so-called Summit Plain receives from geographers most and often sole consideration. In a region of sharp peaks and cirque-walled heights the presence of such extensive highland plain appears, if not a distinct anomaly, at least quite out of place. For adequate explanation we are almost compelled at the outset to seek something else than mere simple epeirogenic elevation of a recent sea-level savannah.

That the Rocky Mountain region is a tract which has been subject to oft-repeated diastrophic movement is a circumstance widely recognized. From earliest geologic times this broad segment of earth-crust appears to have undergone almost continuously both orogenic and epeirogenic disturbance. No less than a dozen times have the ancestral Rockies reared themselves, as majestically, perhaps, as they stand today, only to be speedily planed off to the level of the sea. So frequently does it seem that the region has been a land area that first workers in this field long regarded it as a great continental island lasting quite through all the ages.

In recent years a wholly different view gains credence. It is now generally conceded that sediments of nearly all periodic times have entirely covered the area. Infolded Paleozoics in narrow belts along the Front Range seem to afford ample clue to the former extensive presence of some of the ancient strata.

It is to the later periods of orogenic uprising that attention particularly turns. Since the time when the great limestone plate which saddles the southern Rocky Mountains was laid down in the shallow waters of the epi-continental seas of Mid-Carbonic

days it is certain, according to the latest evidences, that it has undergone several notable deformations.

Each orographic uprising appears to have been immediately followed by profound regional planation. Five such major planation-levels are now on display. Therefore the outlook is not nearly so simple as it once seemed to be. Only by careful consideration of the geologic sequence of events does it appear possible to connect widely separated remnants of each erosion-level; or to correlate the several plains-surfaces.

The most pretentious of all the peneplanations affecting the Rocky Mountain region is the one which took place during Comanchan times. Its areal influence reaches from the Mexican boundary to Hudson Bay and from the Sierra Nevada to the Mississippi River. It is almost continental in extent. It is perhaps the broadest peneplain of which we have knowledge. The position of this old erosion plain is well marked stratigraphically because of the fact that the massive Dakotan sandstone rests directly upon it. By means of the latter all of the later movements connected with the Cordilleran uplift are easily evaluated.

Other peneplanations of the region appear to have taken place, after each orographic disturbance, in Laramian times, in the Miocene, and in Pliocene times. The last uprising seems to be in full swing at the present time and to be acting about as fast as mountain genesis ever goes on. Production of a new base-level is now already initiated. With it enter new and complex complications in the geographic cycle never before even fancied.

It is really beyond the boundaries of the Cordilleran tract that the best evidences of the existence of the several peneplains are presented. The present vertical distance between the First, or Comanchan, and the Second, or Raton, levels is about 4000 feet. Between the Second and the Third, or Maya, plain the interval is approximately 2000 feet. The latter's position is 3500 feet above the existing general plains-surface. What volume of sediments is represented by the successive parts removed through erosion is not very difficult to estimate. In each instance it is a plate not less than one to two miles in thickness.

The level of the Comanchan plain, deformed as it now is, is easily determined in the field by the position of the bottom of the great Dakotan sandstone which reclines upon it. Without going

into details it suffices to note that at the present time it coincides closely with the crest of the Rockies. Only the very highest peaks stand above it. Manifestly the position of the younger peneplains, if remnants could be retained to mark their levels, is far above the existing high points.

In the attempt to refer the remnantal Summit Plain of the Rocky Mountains to any one of the four peneplains of the region about, or even to a hypothetical Fifth plain which might have once stood high above the adjoining Great Plains tract, the Raton peneplain and the Maya peneplain may be at once dismissed from serious consideration. Their elimination is due mainly to the fact that their normal positions are so far distant beyond the tops of the highest peaks. The hypothetical Fifth plain may also be passed over, since its calculated position is really that of the Maya peneplain. The present Great Plains surface seems to be out of question for reason of the fact that it is represented not by a warped surface following closely the broader contours of the Rocky Mountain uplift but by the beds of the existing mountain streams within the elevated area.

Alone there now remains for consideration the possibility of the Comanchan peneplain. As indicated by the attitude of the Da-

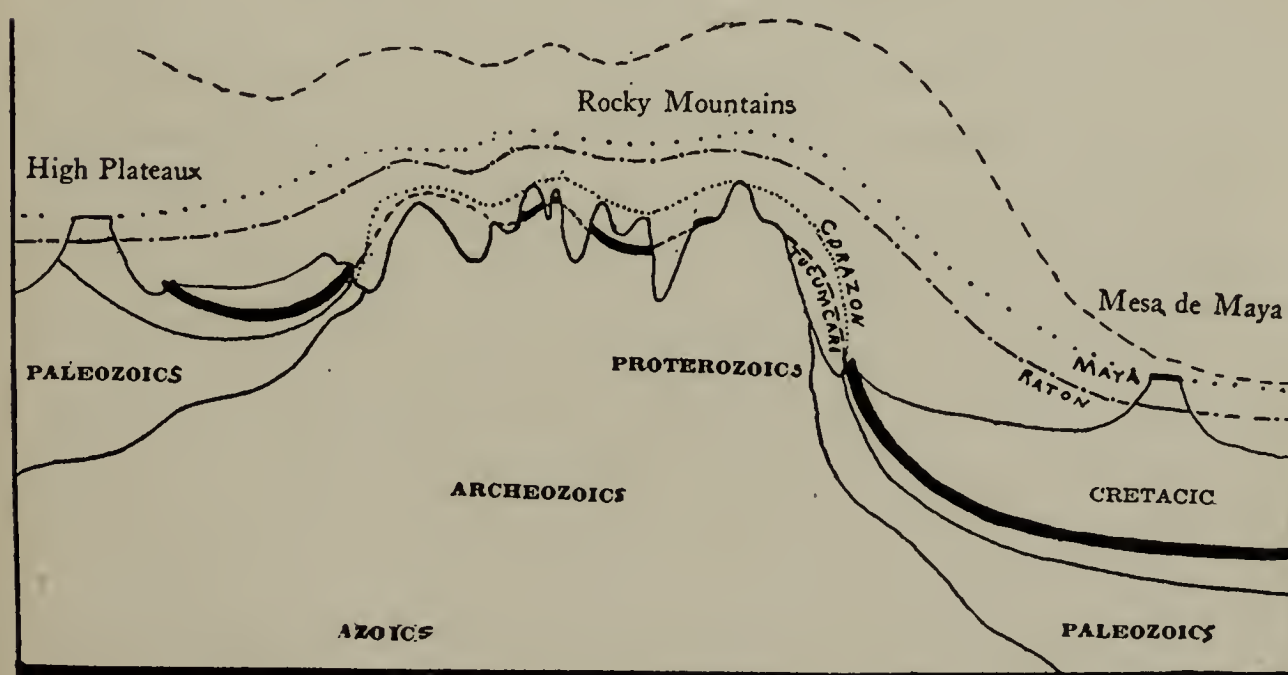


Fig. 21. Peneplains of the Rocky Mountains

kotan sandstone which rises so steeply out of the Great Plains on the flanks of the Rockies and by the position of the remnantal

patches of the sandstone which still persist on the back of the Cordillera, the two coincide. Moreover, it appears that the so-called Summit Plain is present chiefly only near the isolated tracts of the Dakotan sandstone. In some places it is seen to pass beneath the sandstone.

Therefore, it seems very clear that the Summit Plain cannot possibly be the remnantal extension of any hypothetical surface the position of which was formerly high above the Great Plains to the eastward. Neither can it possibly be an uplifted part of the present surface of the Great Plains that has become separated by deformation and erosion. It is apparently a detached area of the old Comanchan peneplain, that has been recently exhumed through the peeling off of the protecting Dakotan sandstone.

There is one thing for which the Front Range of the Rockies is especially notable in a literary way. It is made the theme of extended physiographic description in which geologic features and geologic terms have no essential part. Were it not for the fact that in this method of geographic treatment there lurks great inherent dangers of defective visualization of the true succession of events the plan might find wide application. By ignoring the geologic setting the Summit Plain becomes a section of the newest peneplain, whereas it really appears adjusted more properly to the oldest of the four peneplains of the region. It is an unearthed product of remote Comanchan times rather than a structure originating in the Recent epoch. With this conclusion our ideas concerning the physiography of the entire Rocky Mountain region stands sorely in need of complete revision.

AGE OF TALLADEGA SLATES OF ALABAMA ¹

PROFESSOR WILLIAM E. PROUTY.

University of North Carolina, Chapel Hill

The Talladega or Ocoee section of rocks exposed in Clay County, Alabama, includes chiefly nacreous argillites (phyllites) of varying color, but for the most part gray to green, arenaceous slates, and some quartzites and quartzitic conglomerates. These rocks represent semi-metamorphosed sediments which originally were largely of near-shore origin.

Towards the eastern side of the wide, or main, Talladega area, the dark colored carbonaceous slates increase, there being a considerable thickness of them lying below the thick conglomerates of the Talladega Mountain. Well-defined belts of these black slates of especially high carbon content occur stratigraphically above and to the east of Talladega Mountain.

The total width of the belt of the Talladega rocks, in, and to the west of, Clay County, is approximately 15 miles. This belt is sharply cut off both in the southeast and the northwest by overthrust faults. The strike of the Talladega formation is in most places at a considerable angle with the trend of the faults, so that different sections of the formation occur at different places along the outcrop. Talladega Mountain, in Clay County, is about 7 miles southeast of the westernmost exposure of the Talladega (Ocoee) belt, and the southeastern boundary fault, in Clay County, is usually near the southeastern foot of Talladega Mountain (fig. 22). At one locality, near Millerville, the Talladega rocks have an outcrop width of 7 miles southeast from the Mountain.

There seems little doubt of the equivalency of the Talladega

¹ Published at the suggestion of the Director of the Alabama Geological Survey.

formation of Clay County, Alabama, and a portion at least of the Ocoee formation farther north as described by Hayes² and

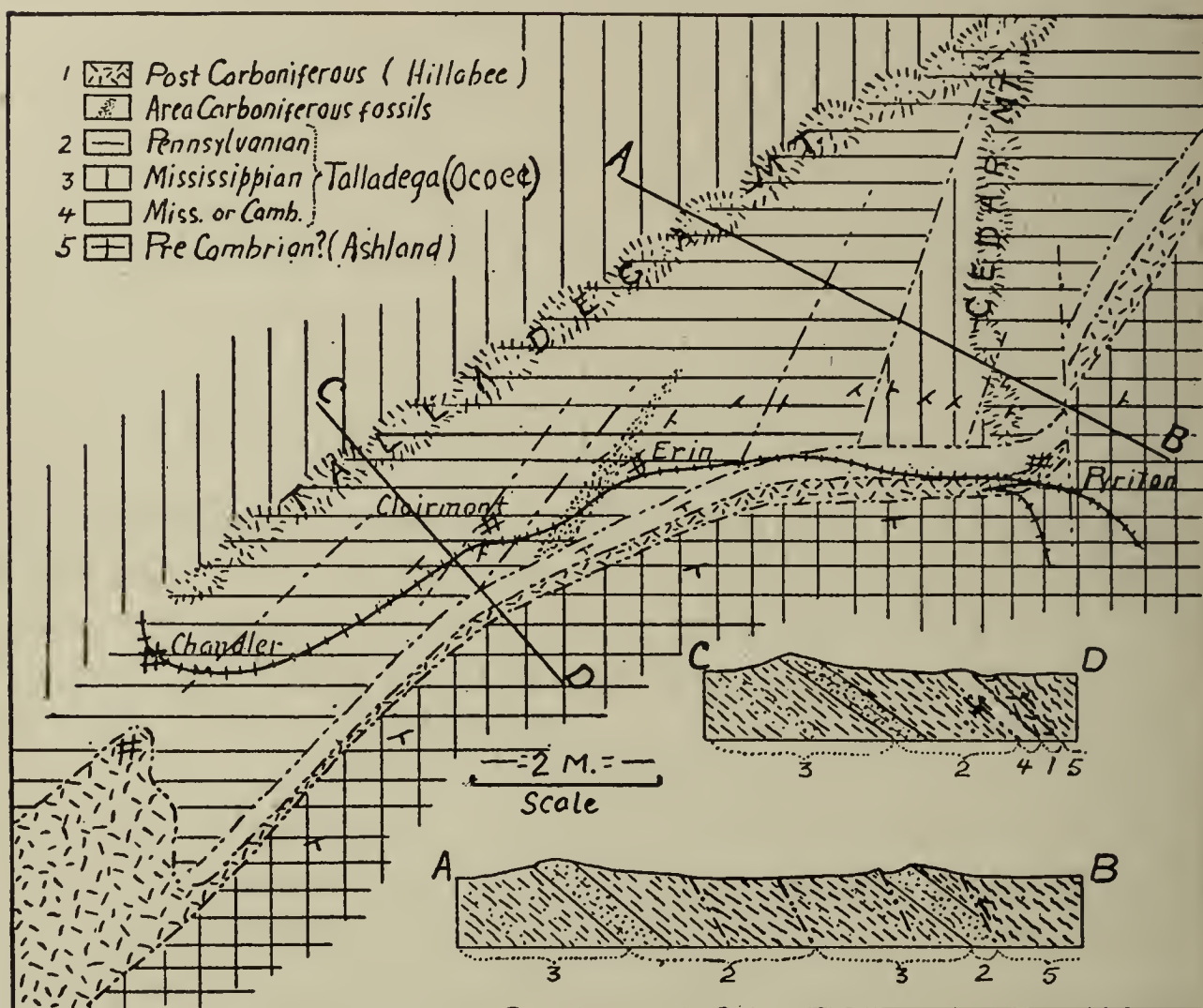


Fig. 22. Talladega Slates Around Erin, Alabama

by Keith.³ This relationship is unquestioned by all who have examined the two areas.

Because of the supposed non-fossiliferous character of the Ocoee formation and its separation from the other strata by faulting, it was difficult to determine its geological age. Classified by many workers in early days as Pre-Cambrian (Algonkian). Safford⁴ originally called it merely "Metamorphic". However, it was long regarded by some geologists that the Ocoee formation was partly at least Cambric in age. In his later report of 1869 Safford referred the Ocoee formation to Potsdam age. Smith⁵ in Alabama considered the western portion of the Talladega (Ocoee) sequence as possible Cambric. Keith⁶ showed the

² U. S. G. S., Atlas No. 20, Cleveland Folio.

³ U. S. G. S., Atlas No. 16, Knoxville Folio.

⁴ Resources of Tennessee, 1st Report, 1856.

⁵ Bull. Ala. Geol. Surv., 1896.

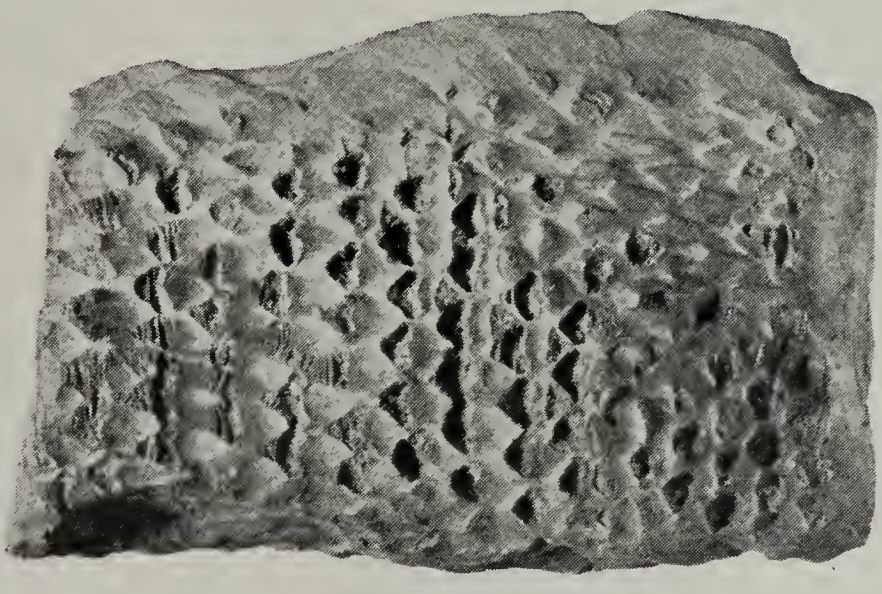
⁶ U. S. G. S., Atlas No. 90, Cranberry Folio.



A



B



C

LEPIDODENDRIDS FROM THE TALLADEGA SLATES OF ALABAMA
A. Flattened siliceous lense. B. Lepidostrobus. C. Lepidodendron Stem

conformity of Safford's Ocoee formation with fossiliferous Cambrian strata and therefore classified it as Cambrian, thus agreeing with Safford. Eckel⁷ considered that the Ocoee formation of the Georgian Dahlonga, district, and the more highly metamorphosed rocks east of them were probably of Paleozoic age. The author⁸ suggested Paleozoic age for portions of both the semi-crystalline and the crystalline areas in Alabama.

The discovery of fossils, *Lepidodendrids* (pl. xxiii) *Calamites* and *Artisias* by Mr. Franklin and Dr. Smith⁹ in black slates near Erin, Alabama, immediately east of Talladega Mountain, in Clay County, and their identification by David White, proved without question the late Paleozoic and probable Carbonian age of a small area of rocks in the Talladega (Ocoee) belt. The question then arose as to the relation of this small fossiliferous area to the rest of the Ocoee rocks. The solution of this problem naturally fell to the author when recently he took up the investigation of the geology of Clay County.

The structural conditions (see fig. 22) show the relation of the fossiliferous area to the rocks for some distance on either side of Talladega Mountain. Although the area about Erin (the fossiliferous area) is considerably faulted, it is quite evident that the fossil-bearing Carbonian rocks overlies conformably and are but little younger than the conglomerates of Talladega Mountain. It is also evident from recent field-work that the strata for some distance to the northwest of Talladega are conformable with these conglomerates.

The black-slate horizon in which the fossils occur at Erin is to be seen in a number of other places in the county. In the vicinity of Millerville the Talladega beds have an outcropping width of not less than 5 miles across the strike and stratigraphically above the fossil-bearing horizon. In this area two other black-slate horizons, higher in the geological column, are known. In neither of these, however, are fossils found.

From a critical comparison of the general sequence of the strata in the Coosa Coal Field, which lies a short distance to the west of Clay County, I am led inevitably to the conclusion that the conglomerate belts of Talladega Mountain and of Cedar

⁷ Eng. and Min. Jour., Feb. 7, 1903.

⁸ Bull. Geol. Soc. America, Vol. xxx, pp. 113, 114, 1919.

⁹ Science, N. S., Vol. xviii, pp. 242-246, 1903.

Mountain belong to the "Millstone Grit" of the Coal Measures, and that the rocks of the Talladega (Ocoee) formation to the east of Talladega Mountain are, with the exception of a few small unfaulted areas, of Mid Carbonic age. The Early Carbonic rocks immediately beneath and to the west of Talladega Mountain extend a considerable portion of the distance toward the westward edge of the Talladega belt, but just how far cannot be definitely told.

It is believed that the western side of the Talladega belt, in this locality, is Cambrian in age, as it seems to be farther to the southwest, near Calera, at which point Cambrian rocks are traceable into the area occupied by the Talladega formation.

In light of the recently acquired facts it appears that the Talladega (Ocoee) formation of Alabama is really made up of a sequence of metamorphosed Paleozoic rocks representing at least three different periodic ages: the Cambrian, the Early Carbonic, and the Mid Carbonic periods.

East of the wide Talladega outcrop, in Alabama, there are several belts of rocks, associated with mica-schists and gneisses which in every way appear to belong to the Talladega formation. These occupy their present position through folding and faulting.

In crossing the eastern portion of the Ocoee belt, in Tennessee, over the newly graded road leading from Chattanooga to Ducktown, the author was forcibly impressed with the Coal Measures aspect of these rocks, and it seemed to him then that the age characteristics of the Talladega formation of Alabama can be largely applied to the typical Ocoee locality. A persistent search for fossils in the black slates, now so well exposed along this newly graded road, might be well worth while; but there, as in Alabama, the fossils are likely to be found only in the siliceous lenses where dynamic forces were unable to totally destroy the evidences of their organic nature.

ORIGIN OF BOLIVIAN COPPER DEPOSITS

BY PROFS. J. T. SINGEWALD, JR., AND E. W. BERRY

Johns Hopkins University

Unlike the romantic quest for the precious metals search for copper or the baser ores was incapable of arousing the cupidity, or of firing the imagination, of Old World adventurers who first penetrated the bleak and inhospitable high altitudes of the South American cordillera that today we know as Bolivia. Three hundred years elapsed after the Spanish reached this region before copper began to attract attention and its systematic mining was inaugurated. Copper mining in Bolivia is, therefore, a relatively recent industry.

Of a series of copper-bearing belts that extend across the Bolivian high plateau from Lake Titicaca to the Chilean boundary and thence beyond in the province of Antofagasta to San Bartolo, Corocoro is the most important. The main features of this mineralization has long been understood; but no detailed or critical inquiry on the ore occurrence such as might lead to a satisfactory interpretation of its mode of genesis has ever been made.¹

The principal mines of the Corocoro district have a longitudinal extent along the Corocoro fault of a little more than 4 km. from the Challcoma mine 1 km. south of the city to the Libertad mine 3 km. north of the city. With the exception of the Libertad mine, the shaft of which is $\frac{1}{2}$ km. east of the fault, all of the mines are located close to the Corocoro fault.

Two prominent cupriferous horizons in the vetas, called the Yanabarra and the Umacoya, parallel the Corocoro fault and can be easily traced on the surface by their outcrop and lines of old workings to where overlapped by the Desaguadero series. In the

¹ For full description of the geological features of this region see the recently published report on "Geology of the Corocoro Copper District of Bolivia," by the authors, in the Johns Hopkins University Studies in Geology, No. 1, 177 pp., Baltimore, 1922.

mines other workable vetas are encountered. No equally prominent ramos cupriferous beds outcrop, although some of the beds outcropping between the Libertad mine and the fault plane are beds that underground yield workable ore.

Mineralization has taken place principally in beds of the Ramos series and the Vetas series close to the fault plane, but also to a limited extent in the fault plane itself. The characteristic manner of mineralization has been the impregnation of sandy beds by native copper. As until about six years ago no facilities were available for working sulphide ores such ores were disregarded and the extent of the sulphide mineralization not realized.

No observations are available to indicate clearly the relations between the sulphide ores and the native copper ores, other than the general statement that the sulphides occur in the vetas near the surface and in depth pass over into the native copper ores and that the ramos ores are all native copper ores. In the Gullatiri Grande mine a streak of sulphide ore is encountered on the third level which is about 200 m. below the surface. In the Remedios mine, the sulphide ores extend to 200 m. below the surface and are there replaced by native copper ores. The upper levels of the Vizcachani shaft yield sulphide ores, but the lower levels are in native copper ores. The Umacoya veta in the Vizcachani shaft carries native copper ore to above the fifth level, a depth of about 150 m., whereas the veta adjoining it in the hanging wall still contains the sulphide ores at this depth. The mines in the ridge north of Corocoro, the Estrella, Copacabana, Capilla, Malcocoya, and San Augustin are producing sulphide ores, but the depths to which they extend are not known.

Ore deposition has taken place principally in arenaceous and pebbly beds and only in the ramos occasionally in shales. Typical native copper ore consists of nearly white to light-greenish sandstone irregularly mottled with specks of copper. A thin section of this ore is made up of rounded to subangular grains of quartz, and a little plagioclase feldspar, ranging from .15 to .60 mm. in diameter and averaging .3 mm. The matrix of these grains is feldspar, chlorite, and a little calcite. The native copper occurs chiefly as a replacement of the matrix, penetrating the boundaries of the quartz grains to a very limited extent, in grains and flakes varying from .1-.3 mm. in diameter and averaging .15 mm. In

some places the copper is regularly distributed through the rock; in other places it occurs in small streaks and patches between which the sandstone is nearly barren. The barren places are sometimes thoroughly bleached but often they are more or less red in color. The alteration products of the native copper ores are cuprite, malachite, and azurite. The ores show wide local variations in richness. The run of mine ores usually range from 2.5 to 3.5 per cent copper. The ore from the Capilla mine averages 6 per cent and the San Augustin is said to have produced ore with 15 per cent copper.

Typical sulphide ore is more highly mineralized than the native copper ore, and the rock is more uniformly impregnated with chalcocite. A thin section of this ore shows that the quartz grains of the original sandstone have been little affected by the mineralizing solutions and ore deposition has taken place by the replacement of the matrix, a process which has proceeded much further than is usually the case in the native copper ores, even to the point of almost complete replacement of the matrix by chalcocite. The color of the sulphide ore unaffected by oxidation is a uniform, metallic-looking gray. Its average tenor is 7 to 8 per cent copper. In the zone of oxidation it alters chiefly to the basic sulphate bronchantite, but also to malachite and azurite; and especially in fractures and along bedding planes, cuprite forms. Hand-sorted mixed oxidized and sulphide ores carry 18 to 25 per cent copper.

The charque, or sheets and arborescent forms of native copper, occurs in fractures, along bedding planes, and in the principal fault planes. The thickness of the sheets varies with the width of the fracture and according to whether gangue minerals are associated with the copper or not. In some cases the native metal occurs as a single sheet completely filling the opening; in other cases it is enclosed in gangue, or is interwoven with gangue. The commonest gangue mineral is gypsum, but celestite occurs in considerable quantity. Celestite also occurs in tabular crystals lining druses in which there has been no copper deposition.

No additional data can be given concerning the occurrence of silver ores, as such ores are no longer produced and silver is a very subordinate constituent of the copper ores now worked. The native copper concentrates, with a tenor of 85 per cent in copper, contain only 6 ounces of silver; and the sulphide concentrates,

with a tenor of 45 per cent in copper, contain only 3 ounces of silver.

An epigenetic origin of the Corocoro deposits and a deep-seated source of the mineralizers was suggested by Baron C. A. de la Ribette.² He considered the ore deposition to have taken place at the time of formation of the Andes, the elevation of which was accompanied by the emission of metallic vapors from the bosom of the earth, which elsewhere produced regular veins, but in this case penetrated the softer beds which they encountered.

The genesis of the Corocoro ores was considered more fully by Forbes.³ He regarded the copper content syngenetic, but its reduction to the metallic state he ascribed to sulphurous fumes emitted at the time of intrusion of the dioritic rocks. Forbes says the problem would have been easier if the deposits could have been shown to have had their cupriferous contents injected into them at the time of the dioritic intrusions as in the case of the copper veins of Bolivia, Peru, and Chile; but he believed the facts in hand point to the copper as originally present in the sedimentary beds, probably not as metallic copper, but in a state of combination, and subsequently reduced to the metallic state. What facts he alludes to he does not specify, and his own description of the occurrence is at variance with such an interpretation. He recognizes clearly that the copper content is confined to the bleached parts of the strata, and he ascribes the bleaching to the magmatic exhalations. Hence the more natural assumption would seem to be that they also introduced the metal. The discoloration of the mineralized rock he concluded was "caused by the evolution of sulphurous fumes, disengaged, and penetrating into the pores of the strata, at the time of the eruption of the dioritic rocks of Comanche and the Cerro de las Esmeraldas, situated respectively to the north and south of the metalliferous district of Corocoro;" and the protusion of these rocks through the Corocoro strata he thought caused the fault and the accompanying dislocations of the strata. More specifically he considered the ore bodies to have been calcareous sandstones impregnated with copper oxide or carbonate. The sulphurous fumes reduced the copper to the metallic form and were themselves thereby oxidized to sulphuric acid. The latter reacting with the calcium carbonate produced the gypsum

² La Gaceta de Gobierno, Vol. LVII, Aug. 2, 1846.

³ Quart. Jour. Geol. Soc., London, Vol. XVII, pp. 7-84, 1861.

which so commonly accompanies these deposits. But it might be added in comment on this suggestion that gypsum is not confined to the mineralized parts of the Corocoro rocks but is quite widespread and abundant in its occurrence beyond the limits of copper mineralization.

Mossbach⁴ thinks that the association of native copper and gypsum can be explained by assuming a basin in which copper sulphate waters came in contact with calcium carbonate. The sulphuric acid attacked the carbonate and formed gypsum and the copper was precipitated as the metal. After the deposition of the first veta, silt carried in by a flood of new waters covered it and protected the copper from oxidation. A repetition of this process gave rise to the various ore beds, and the pressure of the accumulating sediments consolidated more and more the underlying and earliest formed beds of the series.

Sotomayor⁵ attributed the copper of the Corocoro deposits to the reduction of sulphate of copper, which probably represented solutions originating from the decomposition of the cupriferous iron sulphides so abundant in the metalliferous deposits of both chains of the Andes, by ferrous sulphate which in turn would decompose calcium carbonate and produce the gypsum and iron hydroxide which the metalliferous sandstones enclose. Aside from the improbability of the source of the copper sulphate, this explanation has the weakness that the iron hydroxide is least prominent where the mineralization has occurred; and the fact that it explains the deposition of gypsum is no asset for its validity because gypsum, as noted in a preceding paragraph, is in no wise restricted in its occurrence to the mineralized rocks.

Domeyko⁶ makes the somewhat fantastic suggestion that the discordant juxtaposition of the two systems of beds, composed of strata permeable to liquids and united to the two chains of the Andes, would seem to have formed an enormous battery. The sources of emission of electricity would be perhaps the two ranges which enclose metallic substances undergoing decomposition, and the electrodes would be the strata themselves, at the extremities of which the immense deposit of copper has been reduced.

⁴ Die Gruben von Corocoro und Chasavilla un Bolivia (Süd-Amerika), Berggeist, 1873.

⁵ Annales de la Junta de Minería de Copiapo, 1877.

⁶ Annales de Mines, (7), t. XVIII, pp. 531-537, 1880.

Lorenzo Sundt ⁷ discussed the genesis of these deposits at some length. He presents four arguments in support of their epigenetic origin. First, the sheets of copper filling fractures in the beds are naturally younger than the beds themselves. Second, the aragonite twins, replaced in part by copper, must have formed subsequent to the deposition of the beds enclosing them or they would be water worn, and their replacement by copper came still later. Third, the copper occurs not only as a cement but also penetrating the grains and pebbles of the mineralized strata of whatever type of rock they may consist. Hence it was not deposited merely as a filling between the interstices of the constituent particles, but the solutions penetrated the rocks as a whole. Though not specifically stated, the inference from this evidence seems to be that the metal-depositing solutions were more active than ordinary bodies of water in which sediments are laid down, and represent subsequent mineralizing solutions. Fourth, the copper occurs in the Ramos and the Vetaz and hence in rocks of different age. It is more natural to suppose one period of mineralization occurred subsequent to the deposition of the metalliferous beds.

Four other features which he regards as significant with respect to the mode of origin of the deposits are cited by Sundt. First, the copper is generally intimately associated with calcium sulphate and barite, and often so intricately as to predicate simultaneous formation. Second, the unmineralized sandstones are usually red in color, due to the presence of ferric oxide. Where they are mineralized they are bleached through the reduction of the ferric oxide. Third, the cupriferous beds usually contain more or less water characterized by high salinity through the presence of sulphates and chlorides of the alkalies and alkaline earths. Fourth, the position of the ore bodies on each side of the Corocoro fault would indicate some relation between the fault and the infiltration of the cupriferous solutions.

Supported by the above observations Sundt concludes that at some period subsequent to the deposition of the Ramos and Vetaz series, possibly when the Corocoro fault was formed or possibly when the high plateau was uplifted, solutions of copper, chlorides, and sulphates impregnated some of those beds, preferably the

⁷ Boletín de la Sociedad Nacional de Minería, de Santiago, (2a), Vol. IV, 1892.

more permeable sandstones, and where they found conditions favorable deposited the copper. He next raises the question: "What were these favorable conditions?" His answer is quite unsatisfactory. He again calls attention to the replacement of calcium carbonate by native copper in the aragonite twins. The action of cupriferous sulphate solutions on calcium carbonate would explain the formation of calcium sulphate but not that of copper instead of copper carbonate. The latter is explained by assuming the calcium carbonate derived from marine shells. Then the putrefying organic matter contained by the shells would be available as a reducing agent to reduce the copper to the metallic state and the ferric iron of the strata to the ferrous state. The carbonate of iron that would be formed in this way being soluble in carbonated waters would be carried away leaving the mineralized sandstones bleached and colorless. We may suppose further, he says, that the reductive power of the organic materials had not been sufficient to reduce the ferric oxide until it had been aided by the carbonic acid liberated in the reduction of the copper. In this same paper Sundt comments on the "complete lack of fossils" in the Corocoro strata; he can hardly consistently advance a theory of ore deposition based on the presence of marine shells and putrefying organic matter within those beds.

A still fuller consideration of the genesis of the Corocoro ores has been presented by Steinmann.⁸ To him the epigenetic nature of the deposits is beyond question. The ores are not restricted to one or more definite horizons, nor are they restricted to any particular facies of the sediments. They frequently show a vein-like occurrence, and though this is not pronounced, their distribution in the beds is quite irregular. Accepting then an epigenetic origin, he ascribes the peculiarities of the occurrence to peculiar conditions accompanying ore deposition. These were in part inherent in the nature of the mineralizing solutions and in part inherent in the rocks which they invaded. In support of the first conclusion he cites certain characteristics of the copper veins that are widely distributed along the west slope of the western Andes from central Chile to Peru. Most important is the small amount of the common gangue minerals which they contain, from which fact he concludes that the metalliferous solutions to which they

⁸ *Rivista Mineraria*, 32 pp., Oruro, 1916.

owed their origin were relatively deficient in silica, alkali earths, etc. Except in the abundance of gypsum, the Corocoro deposits are analagous to the other Andean copper deposits in this respect. The occurrence of chalcocite, bornite, and domeykite in the Corocoro ores is another point of similarity between them and the West Coast copper deposits. On these grounds Steinmann refers the metalliferous solutions of this district back to the same source and considers them of the same general character as the other cupriferous mineralizing solutions of the Andes.

Why then, he asks, is the copper not united with sulphur and with arsenic as in the West Coast copper veins and why is it in the native state? That the copper was introduced as some salt or sulphosalt will be conceded. Previous explanations have assumed that it entered as a carbonate or chloride, and hence a reducing agent had to be postulated to explain the deposition of native copper. Herein Steinmann believes a fundamental error was made, and thinks the mineralizing solutions were characterized by a scarcity of oxygen, that is, of sulphates, as compared with sulphur and arsenic. In that event, in order to explain the precipitation of native copper, one must seek oxidizing agencies rather than reducing. These, he said, were at hand in the form of ferric oxide of the Corocoro strata. On entering these beds the sulphides of the metalliferous solutions would be oxidized at the expense of the iron oxide and the beds thereby bleached. The resulting sulphuric acid having greater affinity for lime, magnesia, and iron, which it encountered in the beds, than for copper would form sulphates of those elements and the copper would be precipitated in the native state. Iron and magnesium sulphates would be sufficiently soluble to be carried away, the less soluble calcium sulphate would remain. In this way would be explained the bleaching of the sandstone, the formation of gypsum, and the deposition of metallic copper; and the result would be accomplished by solutions of such a chemical character as Steinmann believed formed the other Andean copper deposits. He adds that with an excess of sulphur in the solutions over the available ferric oxide, the copper might be precipated in part or entirely as the sulphide.

The final genetic query raised by Steinmann is concerning the source of the metalliferous solutions. He points out that the

copper deposits of the western Andes are associated with dioritic rocks, usually with granular texture, but in part porphyritic, and are genetically connected with the magmas from which they were derived. Forbes recognized two zones of dioritic rocks, a westerly zone running along the Pacific slope of the Andes and an easterly which extends from the Atacama region through Esmeraldas and Comanchi to Lake Titicaca. Corocoro and the other similar copper districts of the Bolivian high plateau lie in this second zone. Hence Steinmann concludes that the rocks of the dioritic zone exist in depth beneath the Corocoro district and that the mineralizing solutions originated from the same magma. Further it might be mentioned that Steinmann correlates these intrusions in age with the porphyries of the eastern Andes of Bolivia and concludes that the period of intrusion was late Miocene or early Pliocene. If Steinmann was correct in his determinations of the age of the Comanche rocks, then he himself presents evidence against his opinion of the Cretaceous age of the Vetas for they contain fragments of that igneous rock.

Straus⁹ says only that: "the mineralization appears to be due to the reduction effected by organic matter, as well as the replacement of the cementing lime that filled the interstitial spaces in the sandstone."

Douglas¹⁰ does not enter into the question of the origin of the Corocoro deposits but remarks: "it can hardly be doubted that the presence of copper in the metallic state is due to the intrusion of the dioritic rocks."

Singewald and Miller¹¹ consider the close association of the mineralization with the Corocoro fault as indicative of some relation between the two, and think that the parent magma of the diorities was the source of the mineralizers which deposited the ores.

The Corocoro copper deposits are often spoken of as analogous to the Lake Superior copper deposits. These have been more closely studied than the Bolivian occurrence and it would seem that an explanation of their genesis might be applicable to or at least suggest the explanation of the origin of the Corocoro deposits.

A comparison of the geologic features and the ore deposits of

⁹ Mineralogical Magazine, Vol. VII, p. 208, London, 1912.

¹⁰ Quart. Jour. Geol. Soc., London, Vol. LXX, p. 28, 1914.

¹¹ Eng. and Min. Jour., Vol. CIII, pp. 171-176, 1917.

the two districts discloses, despite certain features of similarity, features of considerable difference, and renders it rather doubtful whether reasoning concerning the genesis of one has any direct applicability to that of the other. A close relationship is implied in an article by Alfred C. Lane ¹² on native copper deposits. After mentioning a number of occurrences of native copper ores, of which only the Lake Superior and Corocoro districts are of economic importance, he summarizes the following characteristics as common to them:

1. All occur in connection with red sedimentaries.
2. The deposition of the copper is attended by a blanching of the sandstones.
3. The formation of the red sediments is associated with basaltic dark-colored lavas containing a large amount of ferrous iron and a small percentage of copper.
4. The native copper is associated with waters containing a high percentage of earthly chlorides.
5. The native copper is characteristically irregular and in the nature of a replacement or infiltration of the country rock.
6. Not absolutely universal is the occurrence of zeolites.

It is obvious that these characteristics are more applicable to the North American native copper occurrences than to the Corocoro. Thoroughly applicable to the latter are points 1, 2, 4, and 5. Too little is known concerning the Corocoro mine waters to rule out the probability that their chemical character is merely a reflection of the aridity of the climate and the character of the rocks through which they have flowed and that it has no genetic significance with regard to the ore deposition. The other three common characteristics, 1, 2, and 5, though probably significant are not of fundamental genetic import. Much of the Lake Superior ore is not in red sandstones, hence they were not essential agents in the precipitation of the native copper and they were not the source of the copper-bearing solutions. The Corocoro copper beds extend through a great thickness of strata and the mineralization is so closely related to the Corocoro fault that the ore deposition hardly took place *pari passu* with the deposition of the sediments. Consequently the conditions under which the red beds were formed were not essential to the precipitation of the ores. In other words,

¹² Types of Ore Deposits, 1911.

one is almost forced to the conclusion that the association with red beds is a fortuitous coincidence rather than a significant genetic factor. The blanching of the sandstones indicates that the mineralizing solutions were capable either of reducing the ferric oxide to which they owe their red color to the ferrous state or of dissolving and removing it. No studies of the chemical composition of the bleached and unbleached rock have been carried on to determine which has happened. *A priori*, one might expect most primary ascending mineralizing waters to be capable of blanching red rock by one or the other process, so that the mere fact of blanching tells little concerning the origin or the character of the mineralizers. The occurrence of the ore as impregnations of the country rock is an element of form rather than of genesis and may mean only that the mineralizing solution encountered porous strata rather than open fissures as channels of circulation.

The Corocoro deposits are unusual or anomalous primarily in the occurrence of native copper. But mining developments of recent years have called attention to the large quantities of sulphidic ores and the gradation of the one type of ore into the other, that is, have made less marked the line of separation between the native metal ores and the more common type of copper ores. Experimental chemical work has also demonstrated the ease with which copper may be precipitated from its solutions in the metallic state. Stokes¹⁴ showed that ferrous sulphate will precipitate copper from a solution of copper sulphate and Fernekes¹⁵ that ferrous chloride acts in the same way on copper chloride solutions provided the hydrochloric acid is constantly neutralized. These particular reactions hardly apply to the genesis of the Corocoro native copper, because ore deposition seems to have taken place under conditions that produced a concomitant reduction of ferric oxide in the rocks, but they do show the readiness with which native copper can be precipitated.

Despite many uncertain features and the lack of detailed and exact data, the geologic relations and the mode of occurrence of the Corocoro deposits are now sufficiently well established to rule out all syngenetic theories of their origin. They are due to the impregnation of porous strata along and adjacent to the Corocoro fault by ascending cupriferous solutions. If one could postulate reducing conditions within those strata, the chemistry of the native

copper precipitation would be relatively easy to write. It is true that many of the beds in the vetas contain carbonized plant remains, but they are not coextensive with the cupriferous beds of the Vetass and this material is lacking in the Ramos. Hence it can not be called upon as the precipitating agent. On the other hand, the solutions reduced ferric oxide or dissolved it wherever they deposited copper. Consequently ore deposition took place in the presence of ferric oxide and probably the mineralizing solutions were being oxidized by it. The balance between the deposition of native copper and copper sulphide seems to have been delicate as both were deposited in large quantity. Just what was the chemical character of the mineralizing solutions and just what were the reactions that caused the precipitation of the native copper throughout most of the cupriferous beds are questions that, in the light of present knowledge, can only be speculated on but not convincingly or unequivocally answered. Steinmann's theory of the oxidation of sulphides in those solutions by the ferric oxide and the reaction of the resulting sulphuric acid with the alkaline earths of the impregnated beds, leaves the copper in a state and under conditions favorable to the deposition of native copper. It is the most plausible of the theories that have been reviewed.

The source of the mineralizing solutions may be ascribed with reasonable certainty to an underlying dioritic magma of which the Comanche rock is an offshoot. Evidence of igneous activity during the period of the geologic history of the Corocoro district with which we have had to deal was presented in the account of the geology of the district. The period of mineralization coincided with the period of consolidation of that magma and the mineralizing solutions doubtless bore the usual relations to it which are so generally recognized in the case of epigenetic deposits associated with igneous rocks.

¹⁴ *Economic Geology*, Vol. I, pp. 646-647, 1906.

¹⁵ *Economic Geology*, Vol. II, pp. 580-581, 1907.

NATURE OF VADOSE ORE DEPOSITION

BY CHARLES KEYES

Recognizing the fact, as Posepny¹ has so well urged, that groundwater-level is generally an inclined plane, mine-waters are in consequence continually moving down this slope sometimes through open crevices in the rocks, sometimes slowly through almost impermeable masses.

In its larger aspects vadose ore-formation is comparable in a way to the concentrations of ore-materials on the Wilfley table — the uprising of mountains tilting the former slight incline of the groundwater table and the strata, so that all meteoric waters falling upon the area are directed along certain definite lines. Whenever geologic structures assume the character of cross-folds, faults, or other obstructions to the free movement of the subterranean waters impounding conditions occur and their metallic loads in solution are deposited as ores. The tectonic crossbars are thus the analogues of the riffles of the Wilfley.

In comparison with the precipitation of metallic minerals through impoundment of groundwater all other methods of vadose deposition appear to be insignificant.

In the consideration of groundwater circulation it is of late the custom to regard trunk-channels as chief lines of ore-deposition. So far as the vadose region is concerned this assumption may be seriously questioned. In the Ozark region² for example, it is conclusively shown that trunk-channels are lines of ore-solution and removal rather than of deposition. The Ozark country is in reality a region which is now being very rapidly depleted of its ores. Only when the main channels become clogged in some way or other, do ore-bodies form in them.

It is probably due more than anything else to this fundamental

¹ Trans. American Inst. Mining Eng., Vol. XXIII, p. 213, 1894.

² Trans. American Inst. Mining Eng., Vol. XL, p. 205, 1910.

law of groundwater movement, that mine-waters are always in motion down a slightly inclined plane, that ores are not deposited everywhere evenly in the vadose zone, and that where the movement is interrupted, or stagnation prevails, ore-materials are precipitated or tend to settle down.

Notwithstanding the fact that mine-waters tend to drop their loads into the rock-cavities through which they pass the mineral matter in solution may replace other components in the wall-rock. This chemical interchange may take place along the walls of the channel, or along the lines of stratification-planes, or of joint and fault-planes. Whether mural or stratal in character there are good grounds for assuming that in the vadose zone cavity-filling and wall-replacement are, partly at least, functions of the rate of groundwater motion.

In the light of the most recent tests regarding the locus of maximum ore-deposition the theory of trunk-channel localization of ores, as advanced by Van Hise³ needs to undergo considerable modification before it can be made acceptable. For the greater part of their courses trunk-channels of the groundwater circulation are as already stated, lines of ore-depletion rather than of ore-enrichment. Localization of ores is not due to the fact alone that the deposits are along lines of trunk-channels but mainly to entirely different causes, as is elsewhere pointed out.

One of the most practical results accomplished by the Missouri Geological Survey, during the period when it was in my charge, was the determination of the direct dependence of ore-localization upon certain geologic structures. In the great pitching syncline of Joplin, reaching from the center of the Ozark dome westward out into Kansas and Oklahoma, cross-bars or low arches, trending transversely to the axis of the main trough, were found to mark the locations of all the large mining camps. The ore-bodies were found to be mainly on the up-hill sides of the cross-bars which were high enough to retard, or impond, the mineral-laden waters flowing freely down the open stratification planes of the master syncline.⁴ When carefully investigated, many of the smaller mining camps were discovered to be similarly situated with reference to cross-arches.

³ Treatise on Metamorphism, p. 1202, 1904.

⁴ Trans. American Inst. Mining Eng., Vol. XL, p. 213, 1910.

Later, when the principle was more extensively applied to the mining regions of the West, and of Mexico, it stood every test, even under many novel, complex and surprising conditions. As the basin principle was even more widely extended, it was recognized finally that it mainly controlled vadose ore-deposition, and that its numerous different aspects were ascribable directly to a very few but distinctive geologic structures.

Whatever the precipitating agent, it is certain that when groundwaters become impounded in the vadose zone, conditions of the profound region are imposed. The sulphates in the mine-waters tend to accumulate locally. Organic matter is also present in greater quantity than usual. Kaolinized products are abundant. Altogether the stagnant waters are doubtless, as a rule, very much stronger than running waters in metallic compounds. Under such circumstances ores are much more liable to be readily deposited than is generally the case. Until the local basin is completely filled to the level of its rim with sulphidic ore-materials, or is drained, so that ores again pass into solution, conditions for ore-deposition continue to be remarkably favorable. So distinctive are these conditions that the ores thus formed constitute a class by themselves and should be so treated. Impounded mine-waters are pre-eminently ore-forming.

In the vicinity of volcanic activities the vadose zone is as is well known, the home of various sublimation products, among which are compounds of most of the common metals. These being for the most part soluble, soon pass into the general groundwater circulation. Although they constitute a distinct genetic class of ore-deposits they are seldom commercially important.

The prime function of this class of emanations is their role as an original source of ore materials which are translated to distant points to be deposited. This clue if properly followed should lead to most promising results relating to the genesis of ore bodies.

The association of ore-bodies with specific geologic structures is much more direct and far more intimate than is generally supposed. It is indeed probable that all ore deposits are really thus connected.

In this respect the main function of geologic structure is to direct groundwater circulation along certain restricted and definite lines; and to interrupt the normal movement of groundwater.

This function has much greater significance in the vadose than in the profound zone; and in the arid regions, where the vadose zone is developed to such large proportions, it acquires an importance wholly unknown outside of excessively dry countries.

Broadly considered, all the various original and acquired geologic structures which determine the main conditions of ore-localization may be reduced to the single geometric type of a thin band or belt sufficiently porous in character to permit the ready movement of groundwater currents through it. Whether the band be of the nature of stratification-plane, fault-line, crushed-belt, or cavernous solution-zone, an essential element is its more or less marked permeability. A necessary attendant condition for ore-deposition seems to be that the porous band shall have been recently formed, tilted, folded, displaced or otherwise orogenically disturbed. New relations and new channels of groundwater circulation thus established become the chief factors in new ore-localizing possibilities.

In the vadose zone especially do the new conditions there imposed often give rise to extensive impoundment conditions, where ore-materials in solution may be readily precipitated. These relations of impoundment and ore-accumulation are so closely dependent upon one another and so fundamentally genetic in character that the attendant geologic structures constitute definite and distinctive factors by which vadose ore-bodies may be with advantage, scientifically and practically grouped. The criteria of such classification are particularly useful in the consideration of ore deposits formed under conditions of arid climate; but they are also applicable to ore phenomena displayed in moist lands.

Among ore-bodies mined today it frequently occurs that their origin, often ascribed directly to the action of igneous intrusions because they are in contact, or are near by, is only remotely or not at all connected with such eruptive masses. Were the effects of the latter alone depended upon there would be more frequently no ores mined. That many of these deposits are developed into operating mines is largely due to the fact that in the vadose zone peculiar geologic structures have produced locally favorable conditions for ore-improvement or concentration. It is in the arid regions, because of the deep vadose zone, that the

special features and peculiarities of geologic structures in their relations to ore-genesis are brought out into bold relief.

Commonly, in normally moist climates when ore deposits have been most critically considered in regard to their origin, their vadose relationships are passed over. In an arid climate the vadose ores often constitute the entire workable deposits. If they are at all connected with profound phenomena, the latter are quite unimportant.

Although miners are not always disposed to admit it, it is a well-known fact that they rely much more than they would have us suppose upon geologic structures as clues to ore-bodies. Dependent upon geologic structures as often is the segregation of ore-materials into workable masses, the direct association of the two phenomena is shown at its best only in the greatly broadened vadose-zone of the arid regions. Under climatic conditions of this kind, and where the vadose zone is often more than a thousand feet in depth, so close is the relationship that distinctive types of deposits are establishable based upon geologic structure alone. With the ready determination of the structural type of an ore-body its general shape is at once surmised and the best method of its exploration indicated. This was the end really sought when the geometric form of ore-bodies was thought to be so important a determination in mining.

These structures apply to stratified, igneous and metamorphic rocks alike. In the case of the stratified rock-masses somewhat nicer distinctions are of course possible among the geologic structures producing impondment conditions than in instances of either igneous or metamorphic rocks.

Mining men are prone to attach great practical importance to the shape of ore-deposits, but scientists, especially of late, seem inclined to belittle this feature somewhat, and to emphasize the more obscure genetic features. In the classification of ore-deposits it may be even questioned whether even the cruder distinction according to shape has not, scientifically as well as practically, many advantages over the strictly genetic method. Such plans as those proposed long ago by Waldauf von Waldtenstein⁵, Cotta⁶ Lottner and Serlo⁷, and later by Köhler⁸ and by

⁵ Die besonderen Lagerstätten der nutzbaren Mineralien, I. Band, 1824.

⁶ Lehre von den Erzlagertstätten, I. Band, 1859.

⁷ Leitfaden zur Bergbaukunde, 1869.

⁸ Lehrbuch von Bergbaukunde, 1884.

Höfer⁹ are not without value at the present time. The somewhat similar classificatory schemes of Whitney,¹⁰ Grimm,¹¹ Newberry¹², and Phillips¹³, have important and suggestive features that cannot be well overlooked.

By substituting for geometric shape tectonic form, shape still remains a very useful criterion in ore-classification. In the vadose zone, particularly in arid regions, among deposits of the main genetic group of ore-bodies the distinctions made may be advantageously adopted. To this further attention is directed in another place.

The illustrations noted are those which among others have, in the course of professional work, come under my personal observation. When there are also good descriptions, reference is made to the literature.

Complete cementation of rock-masses is not always an immediate consequence of their first consolidation. Geologic formations may remain porous indefinitely; the Peter Sandstone of the Upper Mississippi Valley has been a good aquifer since Cambrian times. Than this the filling of the interstices of the component grains of a rock is commonly more rapid. Under favorable conditions, of which impondment of groundwaters is not the least important factor, metallic salts form part of the infiltration, and disseminated ore-bodies often result.

When local impondment-conditions prevail in porous sandstones, ore-materials often accumulate, as in the "Silver Reef" of Utah. Elsewhere, through dolomitization of limestones certain layers are rendered quite porous, as in the moist districts of southeast Missouri. The scoriaceous upper surfaces of lava-flows, when buried, constitute layers through which the surface water readily circulates, as in the case of the amygdaloidal copper-deposits of Lake Superior.

In arid regions, sandstones especially are liable to be extremely porous, and in the vadose zone to contain ore-materials in appreciable or even workable volume. This is particularly true of the Red-Beds of western United States, that comprise many thick beds of sandrock. The Red-Beds formations extend over

⁹ Zeitschrift für praktische Geologie, 1897.

¹⁰ Metallic Wealth of the United States, 1854.

¹¹ Die Lagerstätten der nutzbaren Mineralien, 1869.

¹² School of Mines Quarterly, Vol. I, p. 27, 1880.

¹³ Treatise on Ore Deposits, 1884.

vast areas and are everywhere characterized by the presence of copper and other metals. Part of the copper is held by wood and plant remains; but the ores when forming workable deposits appear to be mainly interstitial. In the few cases in which the structure has been made out with this relationship clearly in mind, a well-defined basin has been determined; and beyond its limits where impoundment conditions do not occur there has been no deposition of the chalcocite ore. This is shown in the Hansonberg and Palomas Gap deposits in central New Mexico; along the San Pedro arroyo east of the Sandia range; at Abiquiu, in northern New Mexico; at Copperton in the Zuni district; and about Tecolote, west of Las Vegas, as well as at many other places in New Mexico, Arizona and Colorado. At Tecolote the sandstones are not "red-beds" however, and the distinct faulting on inclined strata has clearly produced local impounding-conditions. Since there are in all these deposits appreciable amounts of silver it is quite probable that the formation of the copper ores is similar to that which I have recently ascribed to the cerargyritic ores of dry regions generally.¹⁴ A like origin seems to belong to the copper-bearing sandstones of the Permian series which lie on the west flank of the Ural mountains in Russia, as described by Krasnopolsky¹⁵; and perhaps also to the Zechstein deposits of Mansfeld, Germany, although not on the grounds advanced by Beyschlag¹⁶. The disseminated coppers of the Corocoro, in Bolivia, appear to belong here, although the mines are upwards of 1400 feet deep, as long ago mentioned by Forbes¹⁷. The Silver Reef mine, in Utah, contains native silver and cerargyrite above groundwater-level and argentite below as interstitial ore-materials in Triassic sandstones, as reported by Cazin¹⁸.

Disseminated ore-bodies may be formed by the distribution of ore-materials through the interstices of rock-components; or they may be due to the occurrence of the metallic salts among the fragments of brecciated belts. Although variously formed, breccias all present a common feature, so far as ore-deposition is concerned. Whether they are formed by crushed masses, as in the case of limestones undergoing dolomitization, by close jointing

¹⁴ Economic Geology, Vol. II, p. 774, 1907.

¹⁵ Mem. Com. Géol. Russie, T. XI, No. 1, 1889.

¹⁶ Zeitschrift für Praktische Geologie, 1900, p. 115.

¹⁷ Quarterly Jour. Geol. Soc. London, Vol. XVII, p. 41, 1861.

¹⁸ Eng. and Mining Jour., Vol. XXXIX, p. 351, 1880.

and fracturing of brittle rocks due to tortional strains, by local pressure-lines without notable dislocation, by faulting, by shearing, or by volcanic outbursts, brecciated beds or belts constituting ore-masses of vadose deposition have the ore-matter disseminated in thin films or sheets between the broken rock-fragments. Rarely does the ore penetrate the latter.

Although brecciation of calcareous rocks arises in various ways, the most prevalent cause is the irregular contraction which such beds undergo during the process of dolomitization. The Hurst iron deposits of Wythe county, Virginia, is an illustration, especially noted by Benton ¹⁹, and is one of the best known.

Close-patterned jointing and fracturing of brittle rocks, mainly due to tortional strains during mountain-making movements, produce open structure whereby metallic salts tend to accumulate extensively in the intermolar spaces thus formed. This is particularly true in arid lands. Usually the rock-mass is a rhyolite, monzonite, or quartzite. The Santa Rita copper-deposits in southwestern New Mexico, are widely known examples. Bingham, Morenci, and Ely in this country, Cananea in Sonora, Spassky in Siberia, and Braden in Chile, seem to be first representatives of the large class of ores known as the porphyry-coppers.

Restricted breccias along lines of jointing often contain considerable precipitations of metallic salts. Many of the gold prospects of the Tueritos and Ortiz mountains, in central New Mexico, quite fully described by Yung and McCaffety ²⁰ are of this sub-type.

Brecciated belts bordering fault-planes are especially notable for their mineral content, which is commonly a strictly intermolar deposition. In the desert, deposits of this character and of strictly vadose origin often extend unchanged to depths of 1000 to 1500 feet, or until groundwater-level is reached. Large numbers of such "veins" have no connection with deep-seated magmas. The Tres Amigos gold-veins, in the Oro Blanco mining district of southern Arizona, present clearly these conditions; as do many of the gold deposits around Animas Peak, northeast of Hillsboro, in New Mexico.

Sheared belts, in metamorphic and eruptive rocks especially,

¹⁹ U. S. Tenth Census, Vol. XV, p. 275, 1883.

²⁰ Trans. American Inst. Mining Eng., Vol. XXXIII, p. 358, 1903.

harbor ore-materials, derived from vadose circulations, as often, perhaps, as they do deposits derived directly from the depths through magmatic emanations.

Volcanic breccias, or pyroclastic layers, are exceptionally favorable beds for the accumulation of ores of vadose nature. They form porous beds long after they are laid down and are often also the abode of metallic sublimates. Typical deposits are displayed in the silver mines of Socorro Mountain, New Mexico.

Whatever the origin of the irregular chambers and passages in the soluble rocks it is certain that they are often filled with detrital materials and ores. Some of these filled caverns are manifestly old channel-ways of subterranean waters which have become dammed; others are enlarged in a vertical direction only; and their fillings long remain because of local impoundment-conditions. Even in the moist climate of the Ozarks, in Arkansas and Missouri, are presented many phases of the phenomenon, as is lately noted²¹. In the arid regions the lead and zinc deposits of Santa Eulalia, in Chihuahua, Mexico, the copper ores of Jimulco, Mexico, the lead ore-bodies of Sierra Mojada, in Coahuila, Mexico, and the gold, silver and lead deposits of the Cave mine, near Mitford, Utah, are good examples. At Santa Eulalia the chambers assume large proportions; for the vadose zone extends to depths of 1000 to 1500 feet.

Inequalities of unconformity-planes produce basins carrying ores much oftener than might be gathered from perusal of the descriptions of mines. Unconformities are geologic structures which in most mining districts commonly escape notice. They frequently are marked by the juncture-line of two diverse kinds of rock. The horizon is usually a porous one and hence is a favorable situation for ore-materials to gather. Not uncommonly a clastic and an eruptive rock are brought together; and the inference is that the ore-body is a contact-deposit.

One of the most instructive deposits of this kind, noted by Winslow²² is in the Doe Run lead-mine in southeastern Missouri. In the Diomea mines, in the Sierra Mojada, lead and copper carbonates and copper sulphides occur under these conditions, according to Malcolmson²³. Blake's descriptions²⁴ of the mala-

²² Missouri Geol. Surv., Vol. VII, p. 673, 1894.

²¹ Trans. American Inst. Mining Eng., Vol. X, p. 203, 1910.

²³ Trans. American Inst. Mining Eng., Vol. XXXII, p. 117, 1902.

²⁴ *Ibid.*, Vol. XVII, p. 479, 1889.

chite ore-bodies of Copper Basin, near Prescott, Arizona, refer to this type of deposit.

Conglomerate-ores form another great group under this heading. The copper deposits of Kelvin, Arizona, may be mentioned. The gold deposits of Altar, in Sonora, Mexico, are well known.

Pitching synclines, or troughs, are among the most important geologic structures with which vadose ore deposits in stratified rocks are associated. In its broader geologic features each mining district is likely to be in a distinct basin. The smaller basins in which ore-bodies are localized are also often of synclinal character.

In moist climates where the vadose zone is necessarily thin, the immediate connection between ore-deposit and trough is not always at first discernible. Yet on the west slope of the Ozarks the great Joplin zinc belt is in the bottom of a wide shallow syncline pitching westward. The main mining camps of the zinc district, Galena, Joplin, Webb City and Carthage, are all located in basins caused by low cross-flexures in the great trough. The Lake Valley silver deposits are all intimately associated with small pitching synclines²⁵. Among the copper-deposits of Bisbee, Arizona, all the larger mines are located in a distinctly pitching syncline. According to Van Hise²⁶ the hematitic ores of the Penokee-Gogebic range south of Lake Superior are similarly deposited in pitching troughs formed by the intersection of inclined quartzite layers and transverse sheets of trap-rock.

Miners are prone to expect ore deposits to occur in connection with faults. For those who are accustomed to ascribe a magmatic source to all ore-materials it is difficult to separate the two ideas. Many ore deposits having no manifest relationships with the depths of the earth are also intimately associated with lines of faulting. Faulting of more or less tilted beds initiates impounding conditions which frequently are the direct cause of ore-localization. A porous layer or band which before may have been the channel of freely flowing groundwater is abruptly cut off or dammed, permitting the ready lodgment and accumulation of metallic precipitates.

An example in which the relationships of the ore-bodies and

²⁵ Trans. American Inst. Mining Eng., Vol. XL, p. 228, 1909.

²⁶ American Jour. Sci., (3), Vol. XLI, p. 117, 1891.

the geologic structures are exceptionally clearly shown is in the Magdalena lead and zinc district in central New Mexico ²⁷. Copper deposits north of Paradise, in the Chiricahua Range, in southeastern Arizona, appear to be similarly formed. The type is a common one throughout southwestern United States.

Under conditions of a normally moist climate numerous cases are found in the Ozark region. I have recently ²⁸ called attention to the Sugar Orchard zinc mines, south of Lead Hill, in Boone county, Arkansas; and Branner ²⁹ has also noted other well-marked instances. On the whole, this type of deposits is not nearly so frequent in occurrence as is generally supposed.

In a single stratum, or geological formation, gash-veins are formed along joint-planes by the contraction of the rock-mass through dolomitization, or through the removal of part of the walls by solution. The Dubuque (Iowa) lead and zinc-crevices are the best known examples, early described by Whitney ³⁰; and more recently by Leonard ³¹. Grant's still later investigations ³² are especially instructive. Many of the Ozark lead and zinc-deposits are of similar origin, although the features are often largely obscured by other phenomena.

In the arid regions the same type of ore-formation is quite prevalent. Among zinc deposits those of Potosi, in the Spring Mountains of southwestern Nevada, are especially noteworthy; they are described by Bain ³³.

So far as vadose deposition is concerned all fissure-vein phenomena may be considered together in this connection. As now generally understood, secondary sulphide-enrichment really applies only to mineral veins originating from the depths. The inference is that the ore-materials from the upper weathered parts of a vein are carried down the vein and precipitated within it at groundwater-level.

At the present time the subject of secondary sulphidic enrichment of ore deposits is probably receiving much more attention than it really deserves. Much more is claimed for the process by its enthusiastic advocates than is warranted. In only a re-

²⁷ Mining Magazine, Vol. XII, p. 109, 1905.

²⁸ Trans. American Inst. Mining Eng., Vol. XL, p. 202, 1910.

²⁹ Arkansas Geol. Surv., Ann. Rept. 1892, Vol. V, p. 33, 1900.

³⁰ Iowa Geol. Surv., Vol. I, p. 451, 1858.

³¹ Iowa Geol. Surv., Vol. VI, p. 36, 1897.

³² Economic Geology, Vol. I, p. 240, 1906.

³³ Bull. U. S. G. S., No. 285, p. 116, 1906.

latively few instances is the enrichment likely to have been derived wholly from the superficial portion of a vein. In the majority of cases the surrounding area doubtless largely contributes the ore-materials. In moist climates vein-stones usually decay more rapidly than the country-rock. In desert lands veins and fault-lines are about the only places where marked chemical change goes on. Very local impounding conditions are imposed on the veins in the same way as elsewhere; but in the vadose zone they are due to geologic structures. Secondary sulphide enrichment of a vein, as generally described, is manifestly due to the fact that the metal-bearing waters are impounded rather than merely intermingled. Examples are innumerable throughout the arid region. The majority of the recent descriptions of secondary enrichment are of this class.

Metasomatic replacement is no doubt a far more important factor in rock-change throughout the vadose zone than is generally imagined. Several of the controlling conditions of metasomatism are usually present in the zone above groundwater-level, although it is only in arid regions that they become prominent. The essential facts regarding the metasomatic alterations associated with fissure-veins have been so admirably and so recently summarized by Lindgren ³⁴ and by Vogt ³⁵ that little need be here added.

In the formation of ore-bodies in the vadose zone, metasomatism processes are probably even more active than in the profound zone. The field is as yet almost untouched. Rickard ³⁶ gives us a glimpse of what some of the changes might be. Moesta ³⁷ describes certain of the features as presented in the excessively dry region of Chañarcillo, Chile. Moricke ³⁸ alludes to other aspects as displayed at Cabeza de Vaca and Caracoles, in the Atacama desert.

In the case of the remarkable zinc-carbonate deposits of the Magdalena lead-district (New Mexico) ³⁹ where entire beds of limestone are replaced by zinc carbonate, every detail of texture is perfectly preserved. Calyces and stems of crinoids, brachio-

³⁴ Trans. American Inst. Mining Eng., Vol. XXX, p. 578, 1901.

³⁵ *Ibid.*, Vol. XXXI, p. 147, 1902.

³⁶ Trans., Vol. XXVI, p. 193, 1897.

³⁷ Ueber das Vorkommen der Chlor-, Brom-, und Jod-verbindungen des Silbers in der Natur, p. 26, Marburg, 1870.

³⁸ Die Gold-, Silber-, und Kupfer-Erzlagerstätten von Chile, p. 27, Freiberg, 1898.

³⁹ Mining Magazine, Vol. XII, p. 109, 1905.

pod shells and corals are now composed entirely of zinc carbonate; and all the delicate surface sculpturings are preserved as sharply as when the animals were alive. For a period of 30 years lead-ores were extracted extensively from these mines until they were thought to be exhausted. That they were also the largest zinc-carbonate deposits developed on the continent was beyond fancy. The zinc-carbonate had replaced the country-rock so completely that the latter was always regarded by the miners as the original limestone. One day an assayer more inquisitive than his fellows undertook to find out just how much metal the country-rock might contain. To his great astonishment he found 40 per cent zinc; and the camp renewed its life as a zinc-producer.

In moist countries where metasomatic phenomena are not commonly recognized in the vadose region, there are beds in which beautiful septate corals are perfectly preserved in sphalerite⁴⁰; and Van Horn⁴¹ notes gasterpod shells replaced by galena.

Metasomatic replacement is to be regarded as a wide-spread phenomenon in the vadose zone. Two distinctions may be advantageously made concerning the replacements by metallic minerals; stratal and mural.

Although it frequently occurs that ore deposits follow definite stratigraphic planes and that certain beds are especially liable to become cavernous, it is not so common to find a given stratum or set of beds entirely replaced by ore-materials. This, however, appears to be the case with certain of the zinc carbonates of the Magdalena district already mentioned. Beneath a well-known local guide-bed called the Silver-Pipe Lime, the zinc ores completely take the place of a particular limestone layer.

The Leadville silver-lead deposits so fully described by Emons⁴² seem to display similar features; as do many other zinc-carbonate deposits recently examined in southwestern United States and northern Mexico. In the present connection more than mere mention need not be made.

The alteration of the wall-rocks by vadose metasomatism is in many respects different from that of individual strata. A number of new factors enter into account. It is noteworthy that quite

⁴⁰ Proc. Iowa Acad. Sci., Vol. X, p. 103, 1903.

⁴¹ Missouri Geol. Surv., 2nd series, Vol. III, p. 97, 1905.

⁴² Monograph XII, U. S. G. S., 1886.

frequently the hanging-wall is much more affected by replacement agencies than the foot-wall. It is to this fact that Rickard ⁴³ calls attention; and to which Moesta ⁴⁴ refers in his account of Chilean silver deposits. Numerous other examples might be mentioned. The subject of mural replacements in the vadose zone requires special elaboration.

Recapitulating, the consideration of the vadose zone as displayed under climatic conditions of aridity discloses a number of novel elements bearing directly upon the genesis of ore deposits generally:

(1). Absence in the arid regions of appreciable rock-decay introduces several hitherto entirely unvaluated factors concerning the formation of the so-called gossans.

(2). Within the greatly expanded vadose zone of dry climates ore-forming conditions, processes and products constitute a distinctive class, comparable on the one hand to those of the profound zone and on the other hand to that on the surface of the ground.

(3). Establishment of well-defined subzones in the vadose region promises to be of the greatest practical value in mine-development throughout the arid regions.

(4). Close association of the form of ore-bodies with tectonic features gives at once a rational foundation for a ready classification of a large group of ore deposits, and in the field a practical clue to their exploration and exploitation.

(5). Clear distinction is to be made between ordinary gossan phenomena of lands having abundant moisture and the somewhat simulating phenomena of the zone above groundwater-level in arid regions.

(6). Impondment of groundwaters within the vadose zone of desert regions, by locally imposing profound conditions, is a determining factor in the localization of many ore-bodies.

(7). Formation of sulphidic ores within the oxidized zone may take place as readily under local impondment conditions as it is accomplished in the profound zone. Only in exceptional instances does oscillation of groundwater-level, probably account for position of sulphidic ore-bodies high up in the vadose zone.

⁴³ Trans. American Inst. Mining Eng., Vol. XXVI, p. 193, 1897.

⁴⁴ Ueber das Vorkommen der Chlor-, Brom-, und Jod-verbindungen des Silbers in der Natur, Marburg, 1870.

EDITORIAL

GEOLOGY IN BOLSHEVIK LAND

When the geologists of the world convened in Russia in triennial conclave as the seventh Congrès International Géologique, vanity was deeply touched by the attentions showered upon them. The greatest Autocrat of modern times was superlatively gracious to the most democratic and independent personages on earth. After entertainment surfeiting and on such sumptuous scale as had seldom before been their lot to indulge these scientists finally set out for home wondering what they had really done to merit such prodigious courtesy. High and low vied with each other to see which could do greatest homage to their foreign guests. Not one of the savants from abroad but who returned to his native heath with a feeling akin to thankfulness that in human evolution intellectual expansion must come from above and percolate downwards, come from the few who are especially endowed with the mind, the wealth, and the initiative to encourage mental development and to further creative productivity. It seemed to be a national trait of character.

Observant ones also saw vast potential powers among the lowly. Politically the leaven of the French Revolution permeated not yet the land of the Russ. Internacine strife might rent the empire; power of the Tzar might be rudely shattered; terrors of foreign war might convulse the nation, but out of it all might come popular rule such as the world had never before seen.

That was a quarter of a century ago. When, one day on that never to be forgotten occasion, while coursing the Euxine, there was of a sudden a mad international scramble among the visiting geologists to see who first should reach the lofty bare peak of the Sudak which rose steeply out of the waves; and as an

American Geologist planted the Stars and Stripes on the needle rock at the top it flashed across his mind that by irony of the Fates a countryman might some day reign from the sacred Throne of the Romanoffs. Little did he dream that in his own lifetime a common, ordinary New Yorker would be happily ensconced therein.

War, revolution, nationalization of wealth, and famine sweep over the land yet the geologist's faith in the final outcome never wavers. To the earth-student, with his global way of looking at things, Bolshevik is not a hated term. It does not mean to him the same thing that it does to potentate and money changer. The torch of the Science Goddess still burns brightly in the home of the Slav.

Through it all the geologist dwells not for a moment on what the Russian might think of him. But Professor Sederholm of Helsingfors tells us in a recent press statement: "To a certain degree the Soviet Government, in spite of its known hatred of the 'intellectuals' has favored those scientists who have been willing, or forced, to serve it as specialists. There is in this State where equality is a watchword, an order of precedence, with a graded scale of thirty-five degrees, of which one is the lowest. Civil Engineers are reckoned in the thirty-fourth class, and certain scientists, as, for instance, the geologists, in the highest one, belonging to the category of 'learned specialist'. Their wages are, of course, falling, as everything else is, because of the rapidly continued depreciation of the money, but at a time when a cord of wood costs 70,000 Soviet rubles, they receive 7200 rubles a month, or 86,500 rubles a year.

"Moreover, the scientist got the special 'scientific ration,' or *outchonnyipayock*, which was a little greater than the daily hunger ration of other citizens. When traveling the specialists could even be allowed a 'Red Guard's ration' which was, however, not always given to them, even in places where stores of food existed.

"The news which we get about the life of the struggling scientists of Russia awakens sentiments of two kinds: Compassion for their extremely difficult situation, and admiration for what they have been able to perform even during such circumstances. One of their greatest sufferings come from the isola-



THE LENA RIVER MAMMOTH

tion in which they live, and therefore their colleagues all over the world are able to relieve a little part of their distress by sending to them, through some safe intermediary, the publications which they have been longing to read during years of seclusion."

Surely when earth-students stand first among the classes, rise first in society, and take precedence over major-generals, for the first time in the history of the world be it said, something good must come out of Isreal.

Russia is sometimes called Red. But "Reds" sought out and returned no less than 47 Rembrants safely to their old home in that most famous art treasury of all Europe, The Hermitage; and they have gathered up the priceless art treasures from the deserted palaces of the Grand Dukes and likewise deposited them in the Petrograd sanctuary. And more thrills; priceless geological treasures are safe. The famous Lena mammoth, that huge hairy elephant, which with its complete skeleton, its flesh, its hide and its flowing hair, still rests unharmed in the halls of the Academy of Sciences overlooking the Neva.

Our Finnish confère goes on to say: "A Russian paleontologist and geologist of great renown, Alexander Karpinsky, the former director of the Geological Survey, was lately reported to be living in great distress, but the last news from him is more comforting. His health has improved, and he is now, as President of the Academy of Sciences, and keeper of its paleontological museum, very active in spite his 77 years."

BEGINNINGS OF ECONOMIC GEOLOGY IN AMERICA

The rise of Applied Geology in this country appears not to date from the time of the attachment of scientific men to the various Governmental exploratory undertakings in the Far West, but to proceed from the special engagement of trained geologists by large corporations and private business interests wherein are sought quantitative and not merely qualitative results, and where returns are measured in dollars and cents.

Curiously enough, the first real impetus given to the commercial application of the principles of geological science was closely linked up with the most exciting political episode of the

time. Beginnings lie far back in the dark causes which precipitated our Civil War. The latter's immediate outbreak is now traceable directly to the deliberate suppression of the geological reports connected with some of the Governmental Pacific Railroad surveys in the early fifties of the last century.

In the decade preceding the outbreak of the Civil War Jefferson Davis was the outstanding political figure in the South and of the Nation. While Secretary of War he conceived the grandiose idea of uniting the oceans by railroad. As revealed long afterwards this project was to rebound immensely to the advantage of the South, and increase the slave states to prepondering influence for all time. Five transcontinental lines were traversed and surveyed. There were three in the South and two in the North. Thirteen sumptuous volumes of Pacific Railroad Reports were devoted principally to extoling the vast natural resources of the southern routes. All notes and records on the northern routes were suppressed, or, as officially pronounced, were "lost in transit".

With the national political conventions just before the outbreak of the Civil War their railroad setting was little regarded at the time. Yet it was really the momentous factor of that day. As the tie that bound together the circle from which its central voice uttered the sentiments of the platform of 1860, this railroad influence was the basic one which precipitated armed conflict within the Nation. As one phase, the most important of all it proved, the Union Pacific Railroad project was then crystallizing so as to be pushed rapidly forward, when the proper time should come, towards the Golden Gate. Its consumation would immediately open to settlement no less than ten new territories, which would soon grow into great and populous states. This single act would place the South hopelessly in the minority in political influence. Heretofore new states were admitted into the Union in pairs — one south and one north. A balance was always nicely maintained. Now, with early completion of the Union Pacific railroad in sight, that balance would be rudely disturbed; and the South could never hope to retain its place in the sun.

Although the official reports on the vast resources of the route were so ruthlessly suppressed from the public this line along the

forty-first parallel proved commercially the most feasible and attractive of all, and it received first financial consideration from private interests when it came to actual construction operations. Despite the obstacles which appear to have been put in its way by the War Department of the Government, the plan of building steadily matured. A northern road accomplished first what the South had designed to reserve exclusively for herself, to open up vast virgin expanses in which slavery had no place. Thus, there was a distinct commercial setting to squatter sovereignty in the years immediately preceeding war times.

Relation of the contest of putting the railroad planks in the national political platforms of 1860 is a long and lurid tale. How it proved to be a rock upon which the Charleston convention split, the one thing which gained the presidential nomination for Abraham Lincoln, and the background for Rebellion is another story.

What the Government could not do, or would not do under Jefferson Davis' guidance, private enterprises now did. The Pacific Railroad Reports were hardly off the press before the engineers of the Mississippi River and Missouri River railroad, now the Rock Island line, then building across Iowa, started a reconnaissance survey from the Missouri River where Omaha now stands to the Ocean at San Francisco. This preliminary survey was completed, and the plans for construction well along when the war storm burst full upon the land. Before the war was ended General G. M. Dodge, who was the chief engineer of the Union Pacific road, had looked up some of the discharged members of the old Northern Pacific Railroad corps. One in particular, Frederick W. Lander, who had been with the Stevens Party over the Great Northern route, had returned from Portland by way of the Snake and Platte rivers. He gave Dodge a full account of the resources and advantages of this traverse and convinced him that it was a very much better route than that with which he had so recently been officially connected.

A little later Dodge got hold of one David Van Lennep, who was reputed to be something of a geologist, to investigate and report upon the commercial prospects of the mineral deposits accessible to the railroad line. Just what position Van Lennep held on the Pacific Railroad surveys, or whether he ever held any post, is not, at this distant day, known. At any rate, Van Lennep

was soon engaged for the Union Pacific Company, and made half a dozen rather elaborate reports. This unique personage was a Fortieth Parallel Survey unto himself. These reports of his on the coal deposits of the Green River Basin, the Bear River Valley, and the headwaters of the Weber River, were especially full and lucid, and they would do credit to present day investigation.

Presently, in 1869, Clarence King came along to make inquiries concerning the railroad's coal lands soon to be exploited. It was presumed that he was really endeavoring to get charge, if possible, of the economic and mining work of the road. Dodge told him of Van Lennep's great efforts and fine results; and gave him copies of the latter's reports. In the subsequent Fortieth Parallel Survey publications King's chapter on the Green River Coal Basin contains nothing of consequence that is not given in full in the Van Lennep's notes. The value of the latter is indicated by the fact that King was so willing to put them into print without material changes in contents except as to the slight additions relating to some general geological observations.

Although entirely unknown to the geologists of this generation Van Lennep's geological work for the Union Pacific Railroad Company marks an initial epoch in the history of economic geology in this country. His reports are sufficiently comprehensive to have been well worth publishing at the time; and they doubtless would have been had the investigations been made under Governmental auspices. That they were not printed was strictly in accordance with private business policy of that day which rigidly prevails to this day. But this work for private interests was so great, so unusual as divorced from Governmental surveys, so meritorious *per se*, that it deserves notice and formal record even at this late date, as an important beginning in Applied Geology in America. The Van Lennep manuscripts repose in the archives of the Iowa State Historical Department. As a pioneer of pioneers in this field David Van Lennep's name should not be permitted to pass into oblivion.

MINERALOGICAL GEOLOGY

Geological Setting of Colemanite Formation. The determination of the origin of the boraciferous minerals of commerce is strictly a geological problem. To be of genuinely practical value in prospecting for new deposits any theory of origin must readily and satisfactorily explain: (1) peculiar occurrence of the thick beds of nodular borate crystal, (2) restriction of the "ball borate" to particular horizons or zones, (3) wide dissemination of finely divided mineral through extensive formations, and (4) idiosyncracies of the characteristic geologic habit of the mineral. No hypothesis yet advanced does any of these things. Main reason is believed to rest in failure to recognize or appreciate fully the underlying principles of desert geology. It is not at all probable that the lime-borate, colemanite, which is now the principal borate of commerce, is always formed in the same way, but that different kinds of deposits and even similar deposits have very different genetic history.

The controlling geological factors are fully a dozen in number. In order of their probable importance they are: (a) nature and age of associated sediments; (b) stratigraphic occurrence; (c) sedimental dependence of borate deposits; (d) anomalous environment imposed by aridity; (f) physiographic setting under desert conditions; (g) direct influence of volcanic activities; (h) possible origin remote from maritime conditions; (i) necessary consequences of chemical affinities; (j) paragenesis of boron minerals; (k) transmutation of intraformational minerals; (l) multiple facies of boron minerals; and (m) severe restriction of localization.

KEYES.

Sedimentary Nature of Colemanite-yielding Deposits. The hypothesis of the metasomatic replacement of limestones by borates,

as advanced by Gale, and his argument for essentially a vein origin of colemanite crystal is, when critically analyzed, a purely academic disquisition, and finds small actual support from observation even as adduced by himself. His basic argument is not so much a judicial marshaling of pertinent facts, elucidating his contentions, as it is a good, although unintentional mis-statement of actual circumstances concerning the stratigraphy of the borate-bearing beds. The upright attitude of the deposits in some places, and the practice of the miners of calling the beds veins seem to have had a potent influence in the birth of the vein hypothesis. Then, too, there appears to be lurking in the argument a certain element of modern commercialism which has no place in scientific writings. The vein prospect appears to be dependent mainly upon the highly tilted position of the formations, and the strata as displayed at Lang, Ventura and Daggett, a feature which does not obtain elsewhere, and which has in reality no genetic significance.

Formation of colemanite crystal as a secondary but perhaps a necessary residue of bittern lakes is not precluded because of alleged absence of other salt beds as is sometimes asserted. These are not always absent as is claimed. In the White Basin sequence there are, besides the colemanite beds, bodies of rock-salt 100 feet thick, and layers of gypsum a dozen or a score of feet in vertical measurement. Boracite crystallizes out long before either of these minerals are thrown down. So, if this supposed feature be used to militate against the bittern lakes conception it at once falls to the ground of its own weight.

The notion that the lava beds associated with the borate deposits are the source of the boracic acid has little to support it. These basaltic layers are interbedded surface flows and not intrusives. Their vesicular upper portions furnish conclusive proofs of their real nature. They could hardly be the original home of appreciable amounts of boracic acid. From wherever the latter comes it is certainly not from surface flows. As volcanic emanations boracic acid ordinarily accompanies some of the dying stages of deep-seated eruptive activities, from solfataras that appear long after lava-flowing ceases.

Ascribing the sedimentary beds which yield colemanite to playa origin presents even greater physical difficulties. At best playas are desert phenomena that are quite ephemeral in character. They

are not, as is commonly assumed, areas of constant and extensive deposition. They are notably tracts of tremendous erosion and removal. The fine blown sediments which are entrapped by the thin sheets of water which spread over such areas for a few weeks or a few days a year, are soon dried and are frequently entirely blown away in a single desert gale. Should any borate be deposited on such bottom muds it would more than likely be speedily deflated and scattered rather than concentrated year after year. In general, playa tracts are areas of characteristic desert denudation rather than intermontane repositories of prodigious soil accumulation.

KEYES.

Contemporary Formation of Commercial Borates. When something over a decade ago the geological setting of the remarkable borate deposits of Death Valley, California, were described their principal formation was tentatively ascribed to normal but successive precipitations of enclosed basins or bittern lakes. This possibility was based partly upon the theoretical behavior of complex salt solutions during progressive evaporation, but mainly upon the observed stratigraphic and lithologic features displayed by the colemanite deposits themselves, as observed in the field. It was also assumed that solfataric action, as noted in the Ash Spring near by, was doubtless an important contributory factor.

The opinion expressed was finally challenged on the assumption that the chief borate mineral, calcium borate, or colemanite, ($\text{Ca}_2\text{B}_6\text{O}_{11} + 5 \text{ aq.}$), being in the form of distinct seams and disposed nearly vertically was essentially a vein formation, boracic acid having acted upon limestone, thereby driving out the carbonic acid. Previously, this possible replacement process had been entertained in connection with the consideration of the Death Valley beds but it was soon discarded because of the utter lack of substantiating evidence.

Colemanite crystal is not the only phase of the formation of boracic acid in nature. Half a dozen other forms occur in the same region. Nor is the genesis of the borate salts all the same. Probably colemanite is unique in its formation. Unquestionably it does not come strictly under the ordinary laws of salts deposition from the waters of bittern lakes, as laid down by Van't Hoff,

Meyerhoffer, Hindrichsen, and Weigat, in which there is a common succession of salts thrown down by the progressive evaporation of saline waters.

Colemanite is doubtless a strictly desert product and is perhaps being formed to a limited extent today in such old playa muds which are covered with boraciferous waters of Ash Spring in the Amargosa Valley, in Nevada, east of Death Valley. These borate-laden waters, soon after gushing forth from the great spring, spread out far and wide over the level playa. The wind-blown dusts carrying much lime constantly settle in the thin sheet of water, or on the mud flat. It may be that the lime borate forms when such spring waters are most abundant, while at other times only fine sands and volcanic ash accumulate.

KEYES.

Interior Seas of the Arid Region. Ever since the time when Whitney, standing on the crest of the Sierra Nevada, gazed out over the desert level of the Great Basin which spread out before him eastward to the very verge of the world, and observed that "No doubt at that [former] time the now arid valleys [plains-level] of Nevada were beautiful inland seas, which filled the spaces between the lofty parallel ridges by which that State is traversed," and that "perhaps the slopes of these ridges were then clothed with dense forests, offering a wonderful contrast to the present barrenness of the ranges, and the monotony and desolation of the alkaline plains at their base," many, if not all, of the desert deposits have been looked upon as old lake beds.

In Whitney's day there was known no such thing as a desert geology. It was not even suspected that the intermontane valleys were only potential lake basins, that none of these countless basins were water wrought, and that few of them had ever been occupied by bodies of water, or that the entire landscape was one of erosion, the prodigious result of normal deflation in a tract from which miles of rock strata had been dislodged and exported on the wings of the winds.

The great lake theory long prevailed. It at length was made to cover the continent. The vast fresh-water Tertiaries formed a theme that fired the imaginations of many an earth student for many a day. Even within the last decade these characteristic clays

and sands of the desert valleys, folded faulted and upturned as they now are, and yielding marine fossils, are still spoken of as fresh-water lake deposits. Because of this fact the reasons for believing that many of them were actually marine beds, and that the waters in which they were laid down were once connected with the Pacific Ocean were recently set forth to such length that the discussion was regarded as an argument for the origin of the desert salines solely from evaporated sea waters.

Tertiaries these beds surely are. They occur in isolated patches of greater or less extent, which stretch out from the Ocean to Death Valley and the Virgen River at the Utah line. At some time or other these remnantal areas were perhaps connected, although not throughout the entire epoch of their deposition. They enclose numerous lava flows comprising a characteristic olivine trap. Some of the layers are possibly of playa origin. Some are undoubtedly old sand-dunes. Many are manifestly bittern lake beds. Clearly homologous throughout their range their affinities are with salt-water rather than with fresh-water.

The history of the salts depositions in these beds is only slightly touched upon as yet. The genesis of the borate ores contained in this thick sequence, for it is upward of a mile in thickness in some localities, is doubtless varied. Some of the salts beds are unquestionably typical bittern lake products. Some may be replacements of calcareous layers. Others of severely limited extent may be transformations in old playa muds. Associated with the borate-bearing bodies are sometimes porous, fluffy and highly calcareous lenses several feet in thickness, beds of rock salt, and extensive gypsum layers. Certain it is, then, that the origin of the borate mineral cannot all be ascribed to a single process.

KEYES.

Marine Origin of Boraciferous Terranes. Genetic basis of borate ore localization finds essential control in the nature of the strata which enclose the deposits. Derivation of the borate measures, conditions of their deposition, and the evidence of their geological age, give ample clue to the geneology of the enclosed ores.

Custom of regarding the intermontane valleys of the Great Basin as the sites of former vast bodies of fresh interior waters is

one of long practice. This is the very idea floridly advanced by Whitney at the beginning of the geological exploration of the Far West. It is a notion which prevails almost unchanged to the present day. Yet, the formations carrying the borate mineral are only a few of those of the region which are commonly denominated as "lake beds." It is mainly because of the fact that the notion has been allowed to go so long unchallenged that recently so much emphasis is laid upon the marine nature of some of these so-called typical lake beds. Stressing this feature somewhat too strongly perhaps, some writers seem to have missed the real purport of the argument, and gratuitously infer that it is a plea for a marine origin of the borate beds themselves. This conclusion is not to be drawn from the memoir in question.

Nevertheless it is a timely question Mr. Strong recently raises in regard to the geologic occurrence of borate deposits. From the viewpoint of the prospector, as well as that of the mine operator, the exceptions to my statements concerning the marine nature of the sedimental succession and the possible origin of the borate beds that he takes is a fundamental one. The evidences which he sets forth and his quotations from other authors all seem to be given in support of his opinion that the borate deposits are of fresh-water origin, as opposed to my suggestion that some of the Tertiaries containing the beds under consideration are without question strictly marine in character. It is well known that nearly all of the literature on the Tertiary deposits of western America regards them as laid down in fresh-water lakes. Indeed, it is the habit to term them "fresh-water Tertiaries." There is really very little foundation for this assumption. All the later and critical data show conclusively that many, if not most, of these deposits were not formed in fresh-water at all; and some of them are not even water-laid deposits.

It is hardly to be regarded that Mr. Strong intended to have his exceptions taken too literally. The basic idea which he possibly meant to convey is that the basins in which the borates were laid down were originally fresh-water lakes, or at least land-locked bodies of water, in contradistinction to their being once arms of the Pacific Ocean. That any of the formations carrying extensive beds of boracite, colemanite, anhydrite, gypsum and rock-salt should be laid down in fresh water seems hardly consistent with

accepted opinion. Sea-water from which 37 per cent of its original volume must be evaporated before gypsum and borates can be precipitated; or 97 per cent of its volume before common salt can form could hardly be termed fresh water. The salinity is many, many times that of the ocean itself. Bittern lakes is a more appropriate name for such inclosed bodies of water.

In the case under consideration the determination of the exact geologic age and origin of the borate beds is of much greater importance than such questions are usually. In the original paper one could hardly give this subject the detailed attention that it merited, or that the recorded observations warranted. Only the main conclusions on these points were formulated; the proofs could be well made to fill twice the number of pages that the entire paper occupies. As these features were purely geologic in character they were purposely reserved for presentation in a more appropriate place.

Singularly enough, the exception which Mr. Strong dwells upon is the very feature concerning which there was taken particular pains to avoid obscurity. The statement that the borate-bearing clays are continuous from the Pacific Ocean to Death Valley, and that they are mainly marine deposits is amply borne out by the fact of the occurrence in them of abundant marine fossils around the Mojave Desert. That these beds are the same as the similarly appearing clays of Silver Peak district, and farther northward and eastward is very questionable. Personal observations, which have been rather extensive, indicate clearly that they are not identical. Beyond Death Valley to the northeastward some of the yellow Tertiaries may be fresh-water deposits at least partly. Quite likely some of them are. However, there seemed to be no critical evidence demonstrating that such is really the case.

The quotations from Government reports which Mr. Strong relies upon for his main proofs are very far from being convincing. They rather weaken the force of his exception. Doctor Spurr is rightly extremely cautious in all of his statements bearing upon this point; he says merely that the Funeral Range beds *resemble* the Silver Peak Tertiaries. The occurrence of a stray molluscan shell of fresh-water type, of "grass" remains, and even of coal-beds in a sequence of strata a mile in thickness does not argue much one way or the other for fresh-water or marine con-

ditions. In the great succession of terranes there are several marked unconformities; marine beds might easily alternate with fresh-water strata. Moreover, there are, overlying the Tertiary deposits in many places, thick clay beds of much later geologic age; the two series often can hardly be distinguished from each other.

There is, indeed, nothing in the quotations which one could regard as undoubted testimony of the fresh-water character of all of the Tertiaries of the region; and it is not probable that any of the authors cited originally intended to give that impression. The keenest discrimination is absolutely necessary in generalizing concerning the Tertiary deposits of the Great Basin. There is no region of like size on the face of the globe in which more confusion exists regarding the exact correlation of geologic deposits. For this reason, if for no other, differentiation of the strictly marine beds from the fresh-water deposits is of the greatest importance to the prospector as well as the miner of borates.

In reality, the application of the term "lake-beds" to all of the Tertiary deposits of Nevada and California is misleading. Notwithstanding the fact that this name is widely used in the reports of the Federal Government and in private publications, many of the so-called lake-beds are as far from being such as are now known to be the vast "Fresh-water Tertiaries" of the Great Plains, between the Mississippi River and the Rocky Mountain front. Positive evidence that any of the Tertiary clays in which borates are now mined are fresh-water deposits is as yet wholly wanting.

The Tertiary clays which contain the borate minerals are in all of their physical characters strikingly like both the loess and the arid adobe soils. Both of these formations are now generally regarded as largely eolian in origin. Present view concerning the Tertiary clays in question is that they are derived from desert dusts blown into the contiguous waters of arms of the ocean, like the Californian Gulf of today, or perhaps in some cases saline bodies of water which finally became nearly completely desiccated. The geographic distribution and limits of the workable deposits of the borates amply bear testimony of this.

If there are really any fresh-water deposits among the boraciferous Tertiaries they should be by all means clearly differentiated from those which are without question marine beds.

KEYES

Vein Character of Colemanite Deposition. Direct evidence of the genetic character of colemanite, of course, lies in the deposits themselves. In natural outcrops relationships are obscure; and the mines present difficulties that are not favorable to detailed study of the character of the mineral. Judgment as to whether these deposits originated as veins or through desiccation involves a study of other more general considerations. Gale's recent hypothesis is thus expressed:

"Artificially colemanite is readily produced by reaction from other calcium borates in saturated alkali chloride solutions; but there appears to be no data concerning the possible formation of this mineral by direct action of boric acid and limestone.

"It is natural to assume that free boric acid, which is a common constituent of many volcanic emanations, might react with limestone, and that by substitution of boric acid for carbonic acid the lime borate would be formed. Thus by a process of metasomatic replacement might be formed deposits of the typical irregular character of the known colemanite masses, roughly following the bedding of the original calcareous rock, or of the interbedded lenticular bodies of limestone. Therefore, in so far as the natural reactions are concerned the formation of colemanite as fissure and replacement vein deposits is well within the limits of possibility.

"The limestone strata with which the principal colemanite ore-bodies are associated are evidently original in the sedimentary sequence. They are in part at least travertine-like, weathering in rough tufa-like surfaces, and are not compact like more typical limestones. Their character suggests the probability that they are chemical deposits, possibly of local extent, laid down in shallow waters, being similar to the travertine deposits now forming near springs or where ground-waters flow into ponds or saline lakes. The limestone masses are believed to be lenticular in form and to occur interbedded with shales at various horizons within the flows of basaltic lava. They are not necessarily the product of desiccation.

"Lastly, an explanation of the source of the boric acid is to be sought. Naturally, a volcanic origin is suggested and often the association of colemanite with basaltic lava is certainly intimate. It is a common saying among prospectors that "borax" will not be found except near a 'porphyry contact' and in association with

limestone. It is true that the larger ore-bodies mined are all in close relation with some part of the basaltic lava-flow rock in place, generally within a few feet of it. The borate-bearing beds follow the outcrop of the basalt flows. The question in what way boric acid originating with the extrusion of these flows could have caused reactions and mineral deposition soon or long after the period of the lava extrusion remains for further investigation.

“Evidence favoring the hypothesis of a desiccated saline lake to explain the origin of the colemanite has little to support it beyond general assumptions. The actual character of the deposits themselves indicates rather a vein type of formation. The gypsum which has been pointed to as a desiccation deposit related to the colemanite is also of vein character. Other salines which would naturally be expected in desiccation deposits resulting from natural salines solutions are not found in association with the colemanite. Those who have supported the desiccation hypothesis have offered no explanation of the reaction which might produce colemanite in such massive deposits as a product of water evaporation, while on the contrary, its formation from limestone in veins by replacement of carbonic acid with boric acid is a natural working hypothesis that deserves experimental investigation. The relations of the deposits to basalt lava flows indicate the probable origin of the boric acid at the time of the extrusion of these lavas, although it may also be necessary to assume that this acid continued to find its way into solution of the circulating groundwaters long after the period of the lava extrusions.”

KEYES.

Vein Attitude of Colemanite Beds. The thick sedimentary succession which carries the colemanite crystal evidently belongs partly, if not entirely, to Eocene deposition. This is indicated also by the tremendous disturbance which the strata have manifestly undergone. In all of the known borate districts the beds are more or less highly inclined; some are vertical; none are still horizontal. Since numerous surface flows of basalt of the olivine variety are also involved the genetic significance is obvious. Moreover, the sedimentaries are very old compared with the other consolidated deposits of the desert; and they perhaps long antedate the desert itself.

At Lang, north of Los Angeles, the borate bed stands on edge, and in view of the circumstances that two thick beds of basalt are associated a peculiar vein-like aspect is easily imagined. At Ventura the strata are likewise so steeply inclined that the impression of a normal vein formation at once presents itself. Calico shafts are nearly vertical. In the Daggett mine-shaft which is on the ore-bed the latter has ten degrees, while the basalt edges of the vertically disposed beds are visible for many square miles over the plain about.

It is doubtless contact only with these occurrences that gave first outline to the vein hypothesis of colemanite formation. But when wider observation is made, and the Death Valley, White Basin and Callville Wash deposits are examined the fancied vein resemblance soon vanishes. It is at once perceived that the borate beds are integral parts of the normal sequence of sediments. Had the last mentioned localities been inspected first it is doubtful whether the vein hypothesis would ever have seen the light of day. In fact Gale, who originated the vein theory, and who most strongly advocated it after his study of the Ventura and Lang deposits, makes no mention of such interpretation when he later visited them and described the Callville Wash localities.

Another mistaken notion is urged in support of the vein theory. Associated with the borate beds which the author mentioned studied are thick plates of olivine basalt that lie parallel to the bed. As stated elsewhere instead of these eruptive masses being intrusive sheets as might be easily inferred, they are really old surface flows, as is conclusively proven by their rough upper surfaces and their vesicular character. Possibility of boracic acid emanating from them to replace the calcium of limestones is virtually *nil*.

To be sure, at Lang and Ventura the surface weathering of the strata has gone on so far as to obscure satisfactory observation. But at Daggett, the even surface of the desert is swept clean by the winds and the irregular edges of the strata stand out as sharply as the deckle leaves of a book. If hasty inspections at Lang, Ventura and Calico leave first impressions that the colemanite deposits are veins, cursory visit to Daggett, White Basin and Callville Wash at once dispels the illusion, and leaves no uncertainty concerning the sedimentary nature of the borate beds. KEYES.

Formation of Borates in Desert Playas. Under the ordinary climatic conditions prevailing in arid lands bittern lakes appear to throw down borate crystal in the form of the soda-lime mineral ulexite ($\text{NaCaB}_5\text{O}_9 + 8 \text{ aq.}$), rather than of the pure lime variety known as colemanite ($\text{Ca}_2\text{B}_6\text{O}_{11} + 11 \text{ aq.}$). This deposition is in accordance with the recent experiments of Van't Hoff, who also endeavors to show that in the subsequent history in order to accomplish the splitting up of the ulexite into its component calcium borate and sodium borate the latter must be removed as fast as it is formed.

In the closed basins of desert playas, with their clay floors, removal of the sodium borate cannot take place to any appreciable extent either by surface or underground drainage, so that the borate accumulations must consist wholly of ulexite and borax ($\text{Na}_2\text{B}_4\text{O}_7 + 10 \text{ aq.}$). When such deposits are later covered over and uplifted sufficiently to allow free drainage from the beds to take place, the percolation of sodium chloride solutions gradually converts the ulexite into colemanite and other members of the colemanite series.

In support of this hypothesis are four lines of evidence: (1) total absence of colemanite and all members of the colemanite series from all playa deposits; (2) occurrence of ulexite in large quantities in some colemanite deposits, as at Lang, California, where it lies for the most part near the foot-wall; (3) bedded character of the deposits; (4) structural features of the deposits, especially the nodular and geodal form of a large portion of the colemanite.

Regarding the last mentioned feature it is to be especially noted that nodular and geodal masses of colemanite are the prevailing deposits at Calico and in some parts of Death Valley and these masses are embedded in the clays. This suggests LeConte's description of the occurrence of the "cotton-balls" in the clays of Rhodes Marsh.

If the hypothesis of the derivation of colemanite from ulexite be accepted, there is a ready explanation of this type of ore. The ulexite embedded in the clays is acted upon by salt solutions. The light, fluffy "cotton-balls" are converted into the more or less compact colemanite, giving rise to the more or less spherical geodes, and allowing the free crystallization of the colemanite in

the center. It is inconceivable that the cavities were originally in the clay, and that they were later filled in with colemanite. Where the ulexite is aggregated in more compact masses the colemanite takes a more massive form, but the contraction of volume still allowed for a large number of drusy cavities such as are so abundant in the deposits. In some deposits, as at Lang, the pressure exerted was sufficient to close the cavities and compact the mass. The selenite, and perhaps some of the limestone also, resulted from the action of sulphated and carbonated waters upon the colemanite.

This appears to be a clear summary statement of the Foshag argument for the playa formation of colemanite.

KEYES.

Faulting of Colemanite Beds. There is one feature concerning the colemanite deposits of California and Nevada which merits especial notice as having critical yet hitherto unrecognized bearing upon the notion of possible metasomatic replacement of limestones by calcium borate, and which is a decisive test of the alleged vein character of colemanite deposits. This is the faulting of the colemanite beds themselves. This structural feature is one which is known to occur in all the borax fields; but it is not noted in

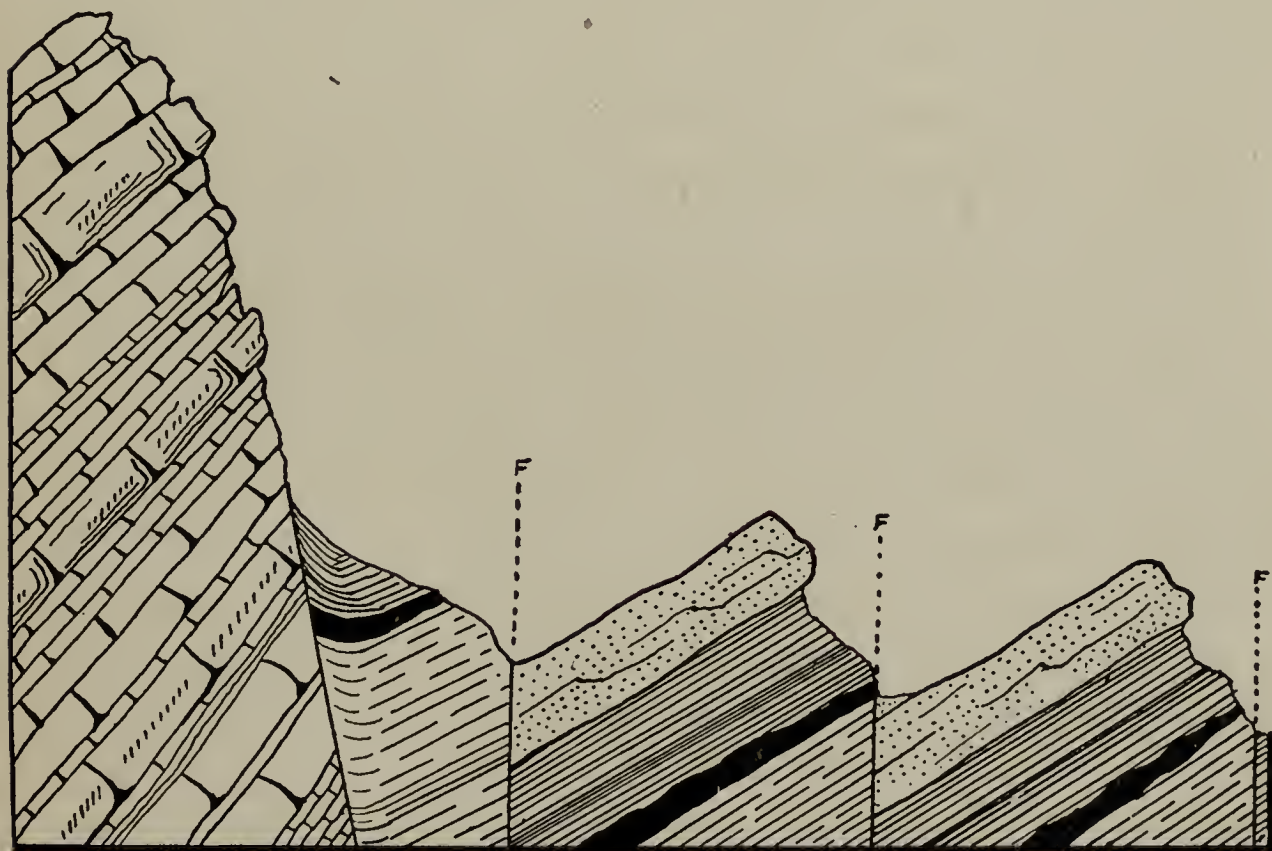


Fig. 24. Faulting of Borate Beds in White Basin

any of the descriptions. It is a phenomenon which is particularly well displayed in several of the borate fields, and especially in the White Basin of southern Nevada. In one instance the inclined strata are shifted horizontally so as to separate the two parts of the borate bed over 300 feet.

The slips are disposed obliquely to the dip and strike, about midway between the two, or about 45 degrees. Although the direction of the movement is horizontal the dislocations are manifestly due to relief of tortional stresses, perhaps set up when the great block now occupied by the White Basin was faulted and depressed 1000 feet or more. These tortional faults sharply cut off both the beds of borate crystal and the associated laminated clays and sandstones. These features are, of course, not displayed at the surface of the ground because obscured by weathering of the deposits and on account of the overwash. But they are clearly shown in the mine tunnels, a hundred yards or so beneath the surface. In the mines the fault phenomena are so clear and fresh that they are fit objects for photography.

Were the borate beds really mineral veins in the true sense of the word they would have been developed as such after the tilting of the strata in which they occur, and at or after, the date of the faulting. There are not the slightest evidences of any of these happenings. The borate beds were without question colemanite crystal at the time of their faulting. Nodules and individual crystals are sharply chopped off by the slipping.

Moreover, there appears to be a secondary deposition of colemanite which doubtless originated from the solution by the rains of the inclined ore-body at the surface of the ground, and which followed down stratification planes. Where the faults were slightly open it was redeposited in a continuous sheet, an inch or two thick, passing directly across the bedding planes of both the original colemanite and the associated shales and sandstones. These vertical slabs of secondary colemanite are easily distinguished at a glance from the original beds. They are composed of fine, long, closely appressed, needle crystals disposed at right angles to the fault-planes, and *ensemble* resemble the thin veinings of satin-spar which traverse the red shales occurring 100 feet beneath the borate beds.

In a single cubic yard of the borate-bearing deposits there are

to be found the massive colemanite crystal that might well recall to mind a limestone bed and at first glance be easily mistaken for such, the clay layers carrying large nodular masses of colemanite, the blue shales through which is disseminate finely divided colemanite, the lamellar clays in which is interspersed paper-like films of colemanite, and the veining plates of needle colemanite cutting across all the others. In the common acceptance of the term, then, it seems utterly impossible for the bedded colemanite to be regarded a vein formation.

If it were possible under existing conditions for borac acid to displace carbonic acid in a normal limestone that had been deformed, tilted, faulted, and broken, it certainly should be in the White Basin that it would have been done. There should also be in the borate-bearing sequence and elsewhere in the borate field other limestone beds that failed to be thus affected or replaced. Of these there appears to be not the slightest trace. Every line of geological evidence thus militates most strongly against the vein origin of the colemanite beds. The vein interpretation of colemanite deposition is an impossible one.

There is one strong feature to recommend the vein theory of colemanite formation. It is almost too foreign and too startling to appeal to scientist. Advocates of the vein theory being something of *doctrinaires* could hardly expect unaided to have such angle of the matter presented to them. It is not a geological phase at all, but a legal and commercial aspect. It opens up a contest royal between lode claim and placer claim that has countless and confusing ramifications. It is well known that most borate claims are located in accordance with the lode rules. Many borate claims, however, are held by placer regulations. These often overlap in a bewildering manner. Frequently the first mentioned claims are plastered over and over by the placer locations. The latter are more comprehensive than the former. They have perhaps better intrinsic rights under present normal conditions. When mining of these shall have been commenced upon these properties there must inevitably and automatically arise immediate conflict between different interests and complicated rights. The prospect becomes gloomy. The outcome bids fair not only to turn into turmoil the entire borax industry of our country, but speedily to accomplish its irremediable ruination. As an American industry

production of borax must entirely cease. South American and Asiatic fields take its place. It is a fight to the death between America and Europe for the control of a world market.

KEYES.

Possible Secondary Character of Bedded Colemanite. When the question of the genesis of colemanite in thick nodular layers of crystal first came up in connection with the Daggett, Death Valley and Lang deposits a likelihood of formation of the borate through replacement of limestone early presented itself. The negative behavior of ordinary chemical reactions, as carried on in the laboratory, offered no serious objections because of the fact that in bittern lakes the multitude of complex salts so often present made almost anything possible. A deterring factor militating against any limestone replacement theory was the circumstance that no regular limestones appeared in close association in the borate bearing deposits; no undoubtable proofs of such a transition revealed themselves; the geological phases of the problem seemed to be all against it; the hypothesis was reluctantly discarded despite the fact that it appeared to be so necessary.

At a later date, the independent revival of Gale's hypothesis of the replacement of limestone, although so spectacular, multiplied so vastly the difficulties previously met with that his arguments for the acceptance of his contention only more strongly clinched the reasons for its rejection. In the meanwhile, another suggestion of origin was made. A proposal for playa genesis of the colemanite through change of original ulexite raised some new considerations. Since beds in which the colemanite occurred were not playa deposits at all the likening of them to the latter was manifestly entirely fanciful. Ingenious as was this argument, and not withstanding the fact that it offered the best evidence against itself, it was the means of casting a new light upon the problem of opening the replacement hypothesis for review.

In the mile-thick succession of sediments in which the colemanite beds are incorporated there is, as already intimated, wonderful diversity of origin. It is doubtful whether any area of like size on the face of the earth presents like variety of formational conditions. Portions, perhaps the most of them, are maritime in character; some are manifestly shallow-water accumulations; bit-

tern-lake beds are in evidence; no inconsiderable number of the lithologic units are characteristically epeirotic or continental in nature; there are many lava flows included; there are rock-salt, gypsum, chemically formed limestones, and volcanic ash beds in rapid recurrence; there are apparently no playa deposits, or anything that approaches them. Judged by this strange changeableness of strata the region was sometimes under the sea, sometimes out of the water, sometimes marshy, sometimes dry land with the water-table only a few feet beneath the surface of the ground. Whether desert conditions of today ever obtained is conjectural, with the probabilities against them.

Under the last mentioned conditions especially a peculiar and perhaps unique situation is often inaugurated in semi-arid tracts and certain spots in the desert. In the dry, thin air excessive evaporation draws the mineral-laden ground-waters to the surface, where, the water passing off, is left a deposit of complex salts. The more soluble salts are speedily disposed of by the infrequent rains. But the lime and less soluble salts often remain behind to form a hard surface layer which Mexicans aptly designate "caliche." The caliche stratum is a few inches to 30-feet or more in thickness. It is essentially an earthy lime-rock, very porous, light, fluffy, and illy bedded. Its notable peculiarity is a characteristic nodular aspect, the individual nodules varying in bulk from the size of a man's fist to that of his head, and even larger, the whole buried in a white limy matrix. Such calcareous deposits form into irregular beds. When excavated by the steam-shovel in railroad grading operations the masses fall readily to pieces and the loaded cars present the appearance of gravel or boulder burdens.

A most noteworthy feature about this nodular caliche is its general structural or textural resemblance to the colemanite beds. Solfataric waters passing over such a surface that caliche presents seemingly could easily alter it more or less completely. With numerous chloridic, sulphatic and other salts also present and the waters carrying boracic, sulphuric and other acids a complex set of reactions must ensue such as rarely or never obtain in the chemical laboratory.

Possible derivation of colemanite beds from caliche obviates every difficulty which attaches to the normal limestone hypothesis.

It overcomes the intrinsic objections of previous formation of ulexite in playas, and especially the almost unsurmountable one that the colemanite is a playa deposit. It harmonizes perfectly with all known geological features. If metasomatic replacement be urged the process must take place while the calcareous formation is still a horizontally lying bed, normally reclining on other sedimental beds. It is, therefore, in no sense a vein formation. By no possible interpretation can it be misconstrued into vein development. The two processes are wholly distinct, unrelated, and incomparable.

KEYES.

Death Valley Section of Boraciferous Terranes. In the consideration of the divers aspects of the colemanite deposits and their origin the great succession of Tertiaries exposed in Furnace Canyon leading out of Death Valley is not without fundamental bearing. The general features are as follows:

	Feet
18. Basalt	50
<i>Unconformity</i>	
17. Shales, gray, boraciferous.....	40
16. Shales, yellowish.....	500
15. Sandstone, pebbly, brown.....	500
14. Conglomerate, irregular.....	60
<i>Unconformity</i>	
13. Shales, argillaceous, yellow.....	200
<i>Unconformity</i>	
12. Basalt	30
11. Shales, pale yellow.....	500
10. Sandstone, friable, reddish.....	25
9. Shales, argillaceous, olive.....	150
<i>Unconformity</i>	
8. Basalt	100
7. Shales, olive green to yellow.....	60
6. Shales, bluish, abundant colemanite.....	50
5. Shales, buff.....	300
<i>Unconformity</i>	
4. Basalt	200
3. Conglomerate	300
<i>Unconformity</i>	
2. Shales, bluish, argillaceous.....	1000
<i>Unconformity</i>	
1. Andesite (exposed).....	500

The suggestion that some of the section might be repeated because of faulting finds no substantiation from observation; yet this is not beyond possibility.

KEYES.

STRATIGRAPHICAL GEOLOGY

Precordillera of San Juan and Mendoza, Argentina. In the provinces of San Juan and Mendoza the Precordillera, a name introduced by Suess, is composed largely of Paleozoic sediments. The tract lies in a north and south line approximately east of Aconcagua, and intercalated between the principal Cordillera of the Andes on the west where marine sediments and eruptive rocks of the Mesozoic predominate, and the region of the Sierras Pampeanas on the east where pre-Cambrian crystalline rocks abound.

The succession of sediments of the Precordillera include those strata which characterize the destroyed continent of "Brasilía," namely, marine deposits of the early Paleozoic succession and the terrestrial beds of the Gondwana group. All of the strata, save those of the upper portion of the Gondwana section have been strongly folded and elevated — supposedly by the orogenic movements of Tertiary time. This Paleozoic structure is, according to Keidel,¹ identical with that of the southern range of the province of Buenos Aires, and the mountains on the shores of South Africa, since in all three regions there exist glacial accumulations at the base of the Gondwana strata as a common characteristic. These accumulations have been discovered throughout almost the whole extent of the region in the San Juan and Mendoza provinces.

The Precordillera occupies a tract 500 km. long, and 60 km. wide. It lies principally between the Rio Mendoza (S.) and the Rio Jachal (N.), and is cut almost in the middle by the Rio Juan. It is clearly delimited on the east. On the north it passes into the mountains of the Desert of Atacama and the Sierra de Famatina. On the south it is joined with the bulk of the Cerro del Plata; and on the west it passes by isolated outcrops to the principal Andean ranges.

It is unnecessary to discuss here the early observations of Bo-

¹ Observaciones geológicas en la precordillera de San Juan y Mendoza: An. Ministerio agricultura Argentina. Sección geología Buenos Aires, 102 pp., 1921.

denbender, Stappenbeck, and Stelzner. Keidel devotes his notes to the region lying between the Rio Jachal and the Rio San Juan. He carries out a minute study of the sediments as to coloration, mega- and microstructure, weathering, and fossil content, giving a number of sections to show the succession of sediments in the region of Cerro del Fuerte, to the east of Jachal, through the Cerro del Agua Negra, a little to the south, and through the Sierra de Talacasto, still farther to the south. In the Talacasto region he finds a change in the character of the sediments. Here a sequence of unfossiliferous grauwacke, sands, and shales, with glacial deposits above rests in some places on Siluric and in others on Early Devonian strata. These sediments were formerly considered Devonian in age despite the fact that the more northern true Devonian beds are abundantly fossiliferous, and for this reason he is inclined to believe that they are probably sediments of Mid to Late Carbonian age. This may be considered unproven.

The structure of the area is complicated, and offers a field for considerable study since comparatively little work has been done on the geology of the region. It appears, however, from the observations carried out that the region was formed by a strong pressure directed toward the east, folding and fracturing the older Paleozoic sediments and thus forming the Precordilleran structures.

R. LEE COLLINS.

Stratigraphic Sequence of Southern Patagonia. The December (1921) number of the *Geologische Rundschau* contains an interesting contribution to the geological literature of Patagonia, by A. Windhausen. The author presents some new observations and conclusions on Late Cretaceous stratigraphy, and general structural elements; at the same time he takes the opportunity to review the results of Stappenbeck, Keidel, and others who have worked in the region.

The Late Cretaceous section is divided into two principal formations, a lower Variegated sandstone, and, conformably above it, the Dinosaur beds. Both are mainly of continental origin, and both contain a marine transgression near their upper limits. The *Areniscas Abigarradas* beds are composed of several hundred meters of compact brown and yellow sandstones which outcrop over a wide area in the territories of Rio Negro, Nequén, and elsewhere. Above this formation there are 200 to 250 meters of

various colored marls and dark clays with boulders, gravel, and dinosaur bones; fossil wood, and plant remains are quite abundant. Glauconitic green and brown sandstone was deposited at the close of the Cretacic time, upon which lie unconformably the marine Tertiary sediments.

The author attributes¹ the change in the character of the continental deposition to either a change in the manner of weathering in the denudation district or to an entirely new source of material. The latter seems improbable. The sandstone at the base is the result of mechanical disintegration in an arid or semi-arid climate, while the clays and marls are the result of chemical weathering under the influence of a more humid climate. Mechanical weathering has its maximum development in areas where there is much frost and rapid temperature changes. The climate would not be especially favorable for vegetation, although observation has shown that it was not entirely lacking. The dinosaur bones are found almost exclusively in the upper member. A moist climate is supposed to have prevailed at that time.

This group of strata is similar in its genetic relations to the Old Red sandstone of England, the Red Beds of North America, the Permo-Carboniferous sandstones of the Karroo formation of Africa, a single member of the German Triassic section, our western Morrisonian series, and the desert sandstones of Australia. Especially is this combination of strata in Patagonia comparable with the Nubian sandstones of North Africa, with which it corresponds not only in general lithologic relations, but also in having a marine transgression in its upper part.

From a critical study of Late Cretacic sedimentation, there is established certain areas along the Atlantic coast which are considered negative elements or depression areas. These areas represent the interior of ancient mountain arcs. The lateral movements of Permian age as Keidel observed them in the southern Sierras of Buenos Aires and in the Pre-Cordillera, extend in the form of an open arc against the Brazilian shield. It is believed that these arcs represent the maximum development, probably, of a force that was in action farther south, although in the present state of knowledge these extensions cannot be proved. However, the movements must have been influenced by the geosyncline be-

¹ Ein Blick auf Schichtenfolge und Gebirgsbau im südlichen Patagonien: Geol. Rundschau, iii Bd., hft. 3, pp. 109-137, 1921.

tween the positive areas. Extrusions of quartz-porphyry and keratophyre of Triassic age are thought to be a result of these movements. The distribution of these eruptive rocks serves to reconstruct, in their essential character, the directions of the

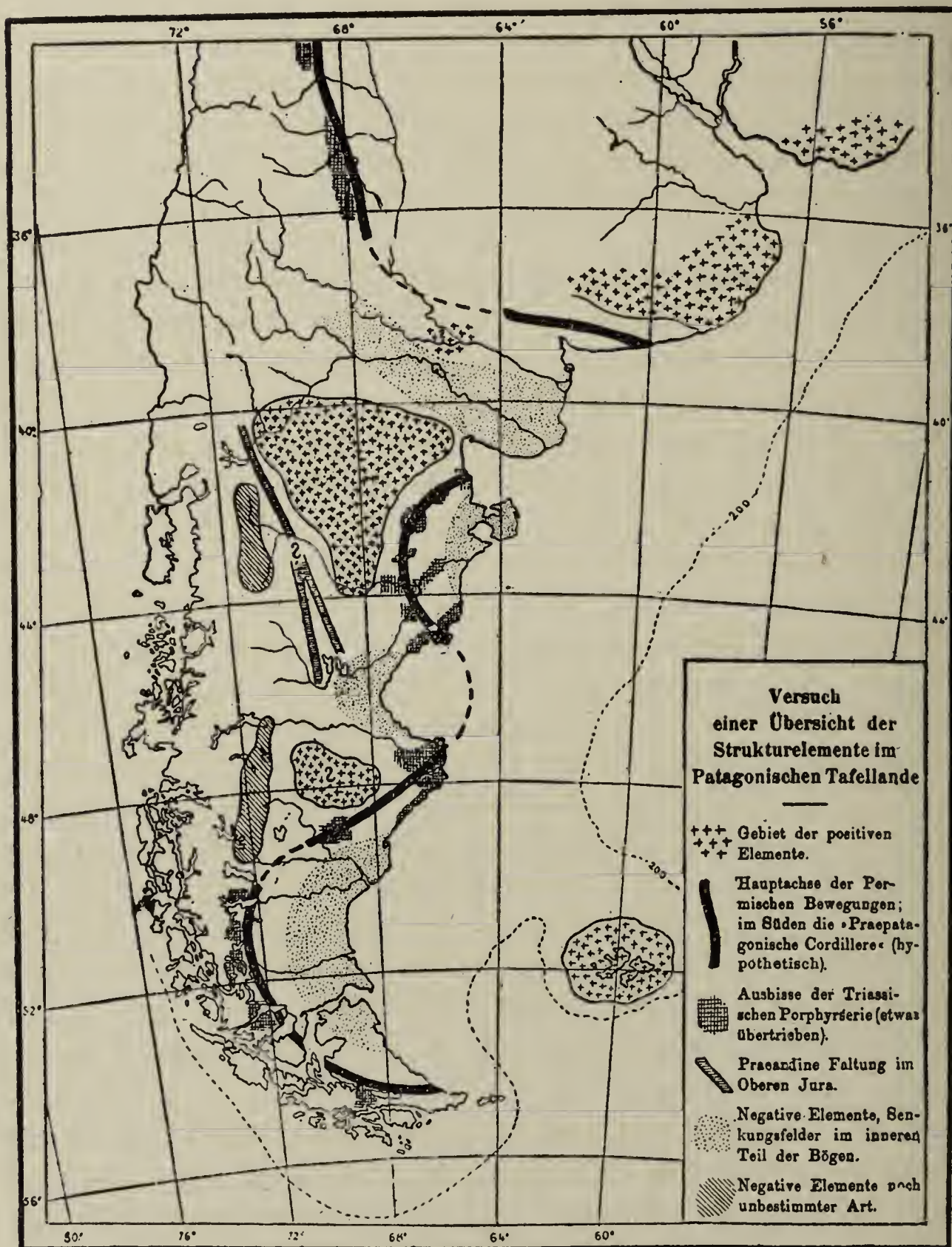


Fig. 24. Structural Elements of Patagonia

Permian movements in the southern part of Patagonia. Following the various exposures of effusive rocks the author reconstructs

his hypothetical mountain arcs which he calls the Pre-Patagonian Cordilleras. The Paleozoic positive areas are characteristically flanked by the arcs and in the interior of these the depressions areas occur.

There are three principal negative elements along the coast. The extent of depression in these areas is not the same. In the southern two there was greater depression at an earlier age.

Farther west there are certain negative elements, the position of which is not so well known, but the fact that they contain lignite beds of Cretacic and Tertic ages shows that a trough existed there during these times.

In various ways the positive areas have influenced the character and phase of the sediments deposited between them in the intermontane depression areas. Their changing state of denudation and elevation determined the limits of the epicontinental sea. When the sea withdrew these positive areas were the source of the materials of the continental deposits. Furthermore, whatever folding followed must have had a different effect between the positive areas and in the positive areas themselves.

A study of the tectonics of the Cretacic and Tertic sediments in the San Jorge basin shows two fundamentally different movements. First, a movement which formed saddles and troughs a few hundred meters across, accompanied by considerable faulting; second, a folding which resulted in folds of greater amplitude, and which can be recognized over a wide district. In the first movement the force acted in a lateral direction; while in the second the forces were caused by the vice-like free ends of the mountain arc. A third phase of movement has forced the west end of the basin up and brought the part along the gulf down until the Tertic deposits are now at sea level.

WALTER R. SMITH.

Basic Tertic Conglomerate of Black Hills. South and east of the Black Hills uplift, in South Dakota and Nebraska, appears a wide-flung mantle of quartz gravels. It caps many hills and occupies a lower position in much of the upland about. Although incidentally noted by Darton and other Government field men, and early associated with the White River beds by Hayden, its exact geological age is a matter of considerable uncertainty. It

is not known whether it should really be regarded as a part of the White River formation, or considered a remnantal bed that might belong to some horizon of the Pierre, Fox Hills, Laramie, Union, or Miocene sections.

In distribution, thickness and composition this gravel bed is quite irregular. Its localization usually in old swales separated on the same level by beds of Fuller's earth, clays, and sands, indicates original stream deposition. In thickness the bed varies from a few inches to a dozen or more feet. Locally there is passage upwards into soft, fine-grained sandstones. The pebbles comprising the conglomerate are mainly the size of a hen's egg, but vary from half an inch to several inches in diameter. Boulders 12 to 20 inches in diameter are not of infrequent occurrence.

This gravel bed, as a rule, is found in remnantal patches occupying relatively large areas, and it is disposed in such manner as to make it almost impossible to prove how it got there. The region over which it occurs is deeply eroded, the span of denudation extending from the Late Tertiary period to the Present time. During Pleistocene times the clays, silts and shales associated near the surface were often extensively reworked.

Regarding the exact position of this gravel bed in the regional geological column the recent evidences appear to indicate that it is an integral part of the Chadron formation. The first locality in which it was definitely and positively discovered in undisturbed position at this horizon was ten miles northwest of Crawford, Nebraska, in the "Bad Lands" on the south side of Big Cottonwood Creek. All over this general region, the Chadron formation lies unconformably upon the "Rusty" member of the Pierre shales. The juncture is nearly always well marked. The lower few feet of the Chadron section here, are a blue-gray silt mixture, which when disturbed remains a long time in suspension in water. Over large areas, this lower section of the Chadron is traversed by numerous chalcedony seams, nodules, and concretionary chunks. The chalcedony is evidently a deposition formed after the beds were in position.

It is in this same lower phase of the Chadron, immediately above the Pierre shale, from a few inches to ten feet, that this coarse, silicious "gravel" occurs. The pebbles do not occur in concentrated pockets as a rule, but are scattered through the silt. When one

notes the nature of the Chadron matrix, it seems impossible that water which had the power to transport such large stones, would not have removed and carried on such fine material with it, or certainly better segregated the very coarse from the very fine materials.

A few miles northwest of Chadron, Nebraska, is a group of hills, weathering rapidly, where the outcrops of the "gravel" is clear-cut and easily seen. Typical Chadron fossils are found in position in it.

The query at once arises what was happening in this area through the long Late Cretacic and Eocene times, while the Laramie and its immediately succeeding beds were being deposited to great thicknesses at very short distances to the west and north.

If the "Rusty" layers of the Pierre shales, or the Ainsworth formation as it is sometimes called, is a true stratigraphic unit and not merely the upper oxidized surface of the eroded and weathered Pierre formation, then there existed a condition wherein neither erosion or deposition occurred for a long period of years, or else that later deposits were superimposed and again removed to a nicety exactly down to the "Rusty" Pierre surface, before Oligocene times set in. Since the Pierre shales are eroded with utmost ease, this suggestion seems wholly untenable. However, whatever were the events during the Eocene time it appears that at the beginning of the Oligocene epoch an interval existed when there were excessive flood activities which distributed large volumes of gravel and large pebbles over a wide, relatively level country.

There is definite evidence of at least one localized stream line of this period which carried coarse, sharp sands and gravels, and which in one area is firmly cemented. The layer shows notable cross-bedding. Erosion has largely removed the marks of this old channel, so that now only remnants remain.

Among the coarse pebbles are found occasionally fragments of a peculiar rose quartz. The only locality at present known where this quartz occurs in position near enough to have furnished the pieces is the area around Custer, among pegmatites in the heart of the Black Hills uplift. If the Black Hills did not already have a protruding core at the opening of Oligocene times, and if they came up after that epoch, as some authors maintain, whence came this rose quartz.

The evidences thus seem conclusive that the gravel bed under consideration forms the basal member of the Chadron formation, and is Oligocene in age. It represents initial deposition of the regional Tertiary succession.

H. J. Cook.

Limitation of Cretacic Formations in Southwestern Iowa. Definite location of important fault lines¹ in Iowa brings about extensive rectifications of many of the previously accepted geological boundaries. Areal distribution of the Dakotan sandstone in the southwestern portion of the State is particularly affected. A very considerable tract formerly mapped as Cretacic in age must now be changed to Carbonic colors. Extensive tongues of Cretacic deposits once regarded as parts of the main body are now known to be merely outlying patches far removed from the continuous mass.

When Lonsdale² was engaged in the mapping of the county of Montgomery, a representative tract in southwestern Iowa as it was thought, and an area that would later serve as a standard for the surrounding districts he was completely nonplussed because of

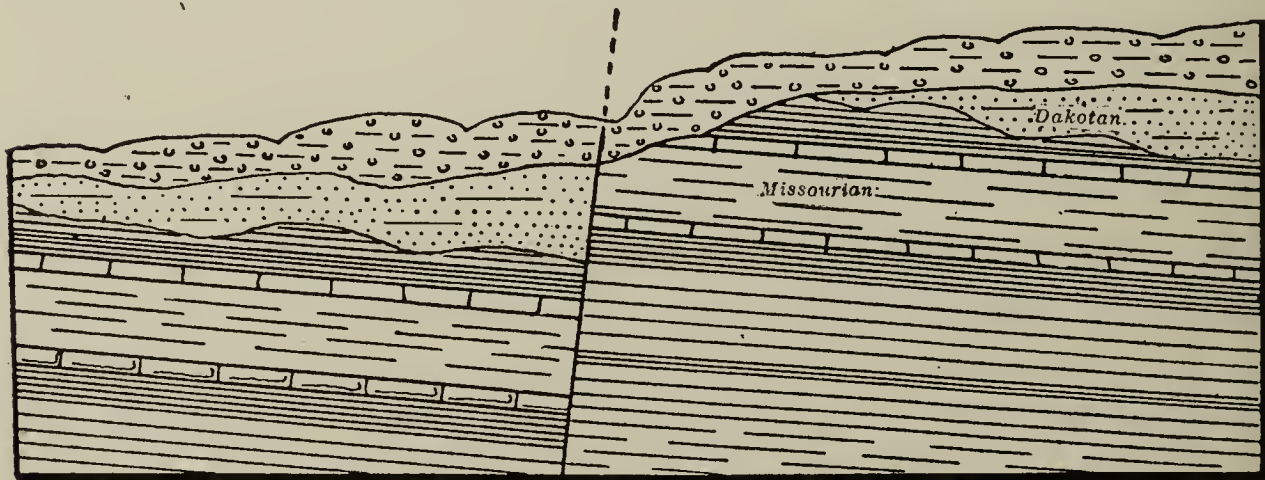


Fig. 25. Red Oak Fault in Southwest Iowa

the fact that in the northern part of the county he was unable to locate a single outcrop of Cretacic rocks. The strata of that age of the southern portion of the district abruptly ceased to show themselves after passing a certain line, a short distance north of the town of Red Oak. Leaving the State after writing the report and in a slightly unfinished condition others filled in the narrow uncompleted belt with Cretacic colors.

¹ Proc. Iowa Acad. Sci., Vol. XXIII, p. 103, 1916.

² Iowa Geol. Surv., Vol. IV, p. 381, 1895.

In the rush of myriads of other things thrust upon the Survey the immediate cause for the disappearance of the Cretacic beds north of Red Oak did not occasion any especial interest. It was long years afterwards that the basic reason was found. On the Missouri River to the westward a disturbance of strata was long known but it was regarded as a fold; but close examination soon disclosed that it was really a fault of considerable throw — about 300 feet. As the dislocation line passed across Montgomery County north of Red Oak it had the Cretacic sandstones on the one side and the Carbonic shales on the other. The Cretacic formation to the south escaped obliteration in the general planation of the region by having been dropped beneath the level of regional denudation.

A wide belt of country north of the Red Oak fault-line is therefore properly to be mapped as Carbonic instead of Cretacic. This broad belt, bordered on both sides by Cretacic formations, extends from beyond the Missouri River northeastward into central Iowa, and completely bisects the Cretacic area. The long southern protruding tongues of Dakotan sandstone which occupy the inter-stream uplands are therefore Cretacic outliers in place of appanages of the main body of the Mesozoic tract.

KEYES.

Rio Grande Carbonic Province. For three distinctive reasons the Rio Grande province of Carbonic rocks recently excites great interest; and assumes an important place in the consideration of later Paleozoic stratigraphy. First, through the Rio Grande sequence of strata the Carbonic formations of the Great Basin region of western America are readily harmonized with the terranes comprising the Standard American section, as represented in the Mississippi Valley. Second, the Rio Grande section of Carbonic rocks is the most complete and the most extensive succession of this age on the whole American continent. Third, a great thickness of beds, nearly 4,000 feet, appear to represent Paleozoic deposition much younger than any other known in America, and perhaps in the world.

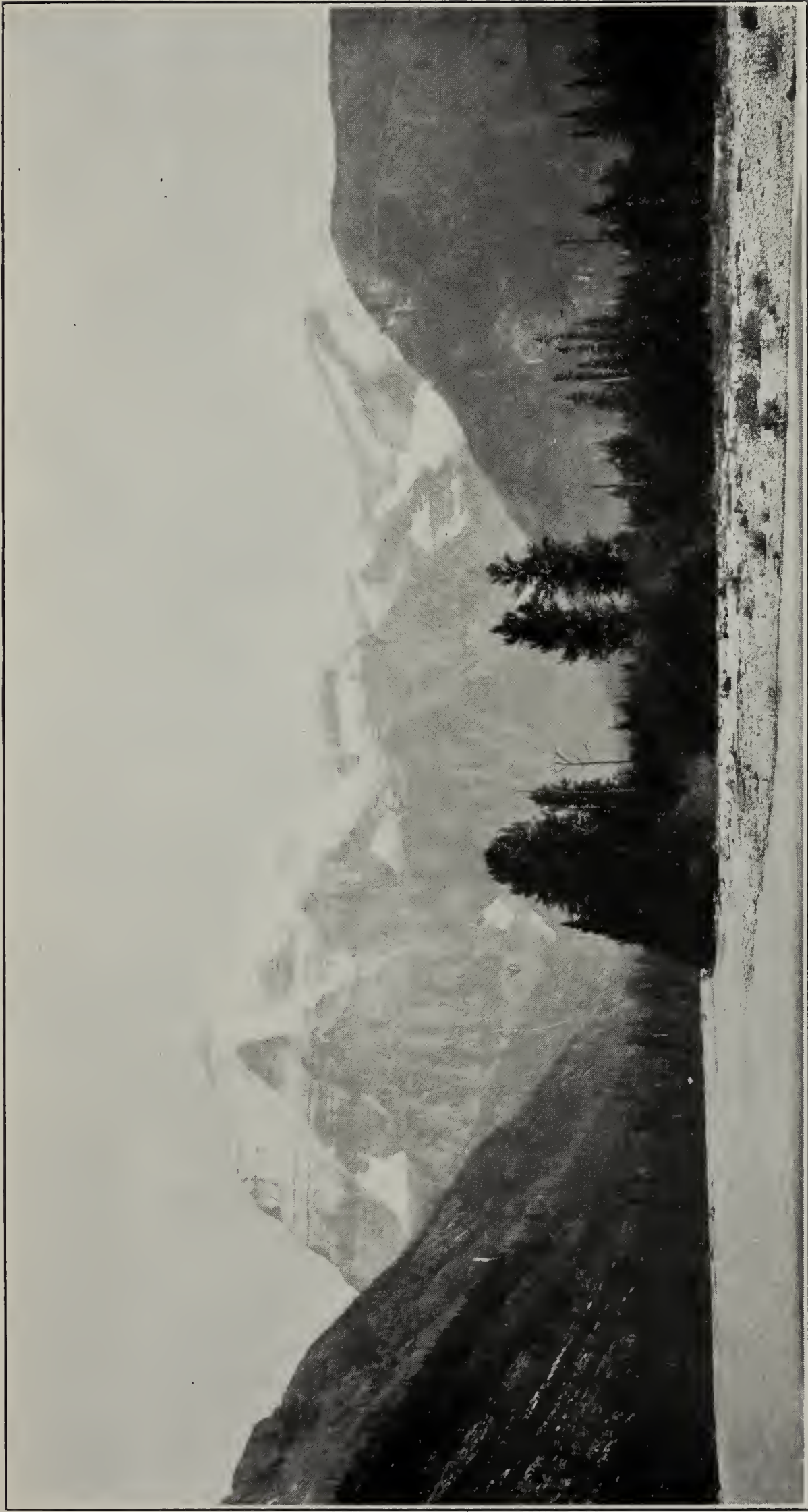
In a recent review of the widely scattered and indifferent literature on the ore deposits of New Mexico there are prefaced sketches of the geologic features of the region. The Carbonic succes-

sion comes in for radical rearrangement, and proposals of new names. It might be questioned whether, without something more than a few weeks' field work, the stratigraphic features of so vast an area may be advantageously made the subject of generalization, or whether even serial correlation is worth while. It is certainly difficult, at a distance to harmonize the various desultory and scattered records, and to weave them into a connected fabric without long familiarity in this field.

The so-called Bernalillo shales, comprising the Abo red beds and the Yeso pink beds, immediately overlying the great limestones of the Sandia mountains, are recognized as belonging to the Pennsylvanian system, as Herrick and others long ago set forth, and not to the Jurassic-Triassic as the Federal Survey has heretofore always contended. The great Maderan limestone is regarded as a compact stratigraphic unit. No red beds of Triassic age are now thought to be represented in the Rio Grande valley; the full evidence of this conclusion had been stated several years before.

Less happily chosen and far from convincing are the various stratigraphic correlations and the correlations of the geologic sections already published. At times the manifest mis-statement of the views and observations of early writers on the region approaches perilously close to carelessness, to say the least. To those already at all familiar with the geology of the region and to those who in the future shall become familiar with it, it must often appear that the correlations made can be only too frequently purely gratuitous. Many instances might be cited. Complete lack of presentation of critical data upon moot points is strongly felt. As to the assumed importance of an attempted explanation of the stratigraphy in some manner or other it is vastly overestimated, largely irrevelant, and, for so broad a field with so limited a time to devote to it, better omitted altogether. At least publication could well be deferred until the data shall have been digested somewhat and the detailed proofs expanded in some other connection.

KEYES.



MOUNT ROBSON CAMBRIC SECTION, TWO MILES THICK

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AND GEOLOGICAL RECORD

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Charles Keyes, Des Moines, Iowa

ASSOCIATE EDITORS:

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